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OBJECTIVE ESTIMATION OF VISUAL ACUITY IN PSYCHIATRIC PATIENTS

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INTRODUCTION

The psychogenic origin of many visual disturbances has long been recognized. Freud (8), Gifford (9), Walsh (19), Duke-Elder (5) and Schlaegel (15) have cited substantial evidence to indicate that symptoms of visual disturbance may play an important role in many psychiatric disorders. Eysenck (6) indicated that psychotic patients may in fact be inferior in dark adaptation and visual acuity to clinically normal people, but acknowledged the limitations of his findings, since soldiers were used as his control group. Another shortcoming in this study stems from his use of the standard Snellen acuity test, which requires good motivation and active participation by the subjects. To complicate diagnostic evaluations further, a recent report by Weekley *et al.* (21) seems to show that certain phenothiazines used as tranquilizers, when given in high dosages over prolonged periods, may produce night blindness, severe loss of visual acuity, or even complete amaurosis.

In the past, many of the reported findings dealing with psychosomatic inter-relationships between visual disturbances and psychiatric disorders have not been considered as specific ophthalmologic problems by psychiatrists. There are, however, at least three important reasons for the participation of the ophthalmologist in the evaluation of psychiatric patients:

1) The ophthalmologic status of "chronic" psychiatric patients is often grossly neglected.

2) In psychiatric patients with ocular dysfunction there is a need for differential diagnosis between organic and psychogenic ocular disorders.

3) The recently reported, possibly adverse psychopharmacologic drug effects on vision require accurate and prompt ophthalmologic evaluation.

To determine the presence of organic or psychogenic visual impairment, development of *objective* evaluation methods is essential.

Visual acuity plays a basic role in vision. With the exception of the previously cited work by Eysenck (6), the literature contains very few experimental studies of possible changes in visual acuity in psychotic patients. However, the well known disorientation, lack of co-operation and poor attention of various types of psychotics may impose serious limitations upon the standard Snellen test procedure.

The optokinetic nystagmus elicited by moving stimuli has been used by many investigators for objective study of visual acuity. The test procedure requires only that the subject look at the drum to assure visual orientation toward the moving test pattern. In this test, the width of the black-and-white stripes which are mounted on a

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rotating drum is decreased until the test patterns fail to elicit an optokinetic response. Visual acuity is determined by the angle subtended by the smallest pattern producing the response. The test has been used extensively for studying visual phenomena by Warkentin and Smith (20) in animal subjects, by Gorman *et al.* (10) with human infants under five days of age, and by Ohm (12) for the objective evaluation of acuity in neurotics, malingerers, or subjects with hysterical blindness. In a comparison of Snellen visual-acuity measures with optokinetic-acuity determinations, Reinecke and Cogan (14) reported a positive correlation of .66 in 100 normal adult human subjects. More recently, Reinecke (13) reported a higher correlation (.91) between the two tests with 26 amblyopic subjects. A higher correspondence between the tests was found in those cases with poorer visual acuity, whereas the disparities between the two tests were greater in subjects with better acuity.

There has been some controversy concerning the supranuclear and nuclear visual mechanisms, and their role in eliciting optokinetic nystagmus in organisms at various phylogenic levels. Based upon earlier observations by Smith and Bojar (17) on subprimate mammals (that optokinetic nystagmus can still be elicited after complete decerebration, but that this response is abolished after lesions in the superior colliculi), optokinetic nystagmus was assumed to be an essentially subcortical visual reflex. This interpretation, however, failed to take into consideration the phylogenic differences in the localization and dominance of the visual centers. More recent reports by Cogan (2), Carmichael *et al.* (1), Tichy (18), Fox and Holmes (7) and Henderson and Crosby (11) indicate that, in man, and probably in most primates, visual cortical areas are essential for the normal optokinetic response. The presence of optokinetic nystagmus clearly indicates the presence of cortical vision. Objective visual acuity thresholds for primates can thus be established by decreasing the width of the test patterns until they fail to elicit optokinetic nystagmus.

The presence of possible disturbances in optokinetic nystagmus in psychotic patients was suggested by observations reported by Diefendorf and Dodge (4) and Couch and Fox (3). Sherman (16) established subsequently that when precautions were taken to ensure the exposure of and visual orientation by psychotic patients to the test striations, the difference in nystagmic frequencies of schizophrenics, although less than those of the controls, did not attain statistical significance.

Since no additional experimental evidence has been reported concerning differences in optokinetic nystagmus between psychotic patients and normal controls, an attempt was made in the present study to compare the standard Snellen acuity with the objective optokinetic nystagmus estimates of visual acuity in psychiatric patients and in normal controls.

Specifically, under the assumptions that the two test procedures yield essentially comparable but not necessarily identical estimates of visual acuity, and that such factors as motivation, active co-operation and attention are considered to be much less critically involved in the optokinetic test, attempts were made to test the following specific hypotheses:

1) That more psychiatric patients should be testable with the optokinetic procedure than with the Snellen test.

2) That in psychotic patients, better acuity scores are obtainable with the optokinetic test than with the Snellen test, assuming equivalence of the two tests in normal controls, and that under these conditions, differences between the Snellen and optokinetic acuity scores should be found to be greater in the psychotic patients than in the controls.

3) That in a comparison of the optokinetic acuity scores of psychotic patients

with those of normal controls obtained with the same objective optokinetic test, no significant differences in visual acuity should be found.

METHOD

Subjects: The experimental group consisted of 437 female psychiatric patients ranging from 18 to 91 years of age. The categories used in the analysis included 247 schizophrenics, 102 chronic brain syndromes, 18 manic depressives, 18 involutionals, 15 lobotomies (of various diagnoses), 12 psychotics with mental deficiency, and 25 other unspecified psychotics. Length of hospitalization, and duration of present or former drug or other type of treatment, did not enter into the selection of these patients. The control group, made up of both males and females, consisted of 84 hospital staff members, their ages ranging from 18 to 65 years.

Apparatus: The standard Snellen acuity chart was used to determine subjective acuity. The optokinetic apparatus consisted of a translucent plastic cylinder, 10 inches in diameter and 12 inches high (Figure 1). A rigid base supported a gear system and a vertical shaft on which the cylinder was rotated by an electric motor.

The test patterns consisted of sets of vertical black and transparent striations of equal width printed on clear film. The test pattern line widths, test distances and resulting acuities used were, respectively:

0.023 in. at 2 M., 20/20; at 1 M., 20/40
0.058 in. at 2 M., 20/50; at 1 M., 20/100
0.23 in. at 2 M., 20/200; at 1 M., 20/400; at ½ M., 20/800

The selected test pattern was mounted on the outside of the translucent cylinder and transilluminated by a diffusing tubular incandescent light source located inside the cylinder. The level of illumination for the test pattern was set to approximate the level used in the Snellen test. The rotation of the cylinder was controlled by a rheostat and

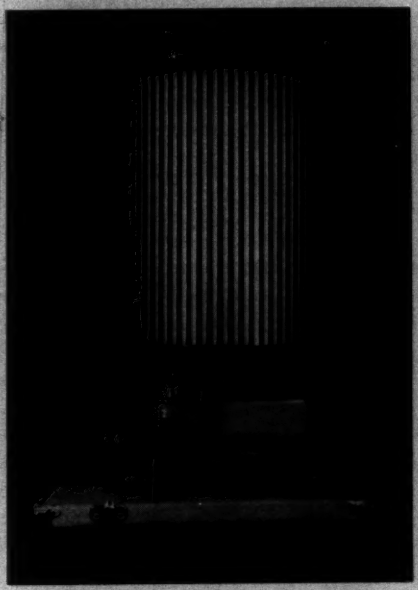

FIG. 1. Optokinetic apparatus with test pattern.

was adjusted to optimal speed for each stimulus, not exceeding 40°/sec.

Procedure: The Snellen test consisted of the standard procedure for determining acuities in each eye separately. Acuities less than 20/800 were designated 20/1200 in the calculations.

In the optokinetic procedure, the subject was seated first at 1 M. from the drum and then at 2 M. until the smallest test pattern which elicited nystagmus was noted. If nystagmus was not observed at 1 M. with the largest pattern, the subject was tested at ½ M., and if no response was elicited, an acuity of 20/1200 was recorded. The test distances and test patterns used were selected to correspond to the thickness of the Snellen letter-lines, so that equivalent Snellen notations could be used for specifying acuity measures. The optokinetic responses were recorded as positive when the moving test pattern elicited a prompt and properly directed rhythmic ocular response in both directions of drum rotation. A negative response was recorded when the elicited

eye movements were not well sustained, infrequent, poorly defined or absent. Each subject was tested with both acuity test procedures on the same day, and each examiner was unaware of the results obtained by the other.

RESULTS

In view of the similarity of results for the two eyes, only the visual acuity data from the right eye were used in the statistical analysis. Uncorrected visual acuity scores were used.

For an evaluation of the first hypothesis, a comparison was made of the number of subjects not testable with the Snellen test and the number not testable with the optokinetic procedure (Table 1). Those not testable with the Snellen test were uncommunicative, unco-operative, could not understand the instruction, or did not read. On the optokinetic drum, subjects who closed their eyes, would not look at the drum or looked down or to the side were counted as untestable.

As is shown in Table 1, all control subjects were testable by both methods. In the psychiatric group, a significantly greater number of subjects were testable with the optokinetic method than with the Snellen test. This was true for schizophrenics, chronic brain syndromes and "other diagnoses," separately, as well as for all psychiatric patients taken as a group. (One-tailed probabilities from the sign test were used, since the hypothesis predicted the direction of the expected differences.)

TABLE 1

Per Cent Not Testable with the Snellen Test or the Optokinetic Drum by Diagnostic Categories

Diagnosis	N	Per Cent Not Testable		p
		With Snellen	With Drum	
Controls	84	0.0	0.0	—
All psychiatric subjects	437	19.2	5.9	<.0001
Schizophrenics	247	19.0	6.1	<.0001
Chronic brain syndromes	102	18.6	6.9	<.001
Other psychotics	88	21.3	4.5	<.001

To test the second hypothesis, the mean acuity scores obtained by the two procedures were compared. Geometric means were used. Subjects untestable by either measure, and, in addition, 18 psychiatric subjects who were tested only with glasses, were excluded from these comparisons. The numerator of the Snellen ratio, 20, was omitted in each case, so that the denominator served as the acuity score for purposes of calculation. Snellen test scores for which a corresponding optokinetic score did not exist were recorded as the next poorer acuity obtainable with the drum—*e.g.*, a score of 20/30 on the Snellen was treated as 20/40.

The results are shown in Table 2. The mean Snellen scores in all groups were significantly higher, indicating poorer acuity than in the scores obtained with the optokinetic drum. Although the Snellen-drum differences in acuity were greater for the psychiatric patients than for the controls, these differences were not statistically significant. This was tested by a chi-square between the frequency of greater Snellen or drum acuities, by subjects, in psychiatric patients and controls.

TABLE 2

Mean Acuity Scores on the Snellen Test and the Optokinetic Drum by Diagnostic Categories

Diagnosis	N	Geometric Mean, Snellen	Geometric Mean, Drum	p
Controls	84	51.0	39.2	.035
All psychiatric subjects	327	81.9	58.5	.011
Schizophrenics	189	72.3	55.3	<.0001
Chronic brain syndromes	73	113.8	67.5	<.0001
Other psychotics	58	81.1	58.9	.025

To evaluate the third hypothesis, the mean optokinetic acuity scores for the control and the psychiatric groups were com-

TABLE 3

Mean Acuities of Psychiatric Patients and Controls on the Optokinetic Drum

Age	Geometric Mean, on Drum					
	Controls (N)	Schizo. (N)	Chronic Br. Syndrome (N)	Other (N)	Psychotics (N)	*p*, Psychotics vs. Control
Under 30	43.9 (36)	45.4 (9)	— (1)	— (1)	45.3 (11)	>.05
30–49	36.4 (32)	42.4 (92)	51.3 (9)	62.4 (17)	46.1 (98)	>.05
50–69	34.8 (15)	64.0 (92)	68.7 (30)	46.8 (26)	61.5 (148)	.01
70–	— (0)	85.2 (16)	74.1 (33)	76.9 (14)	77.5 (63)	—

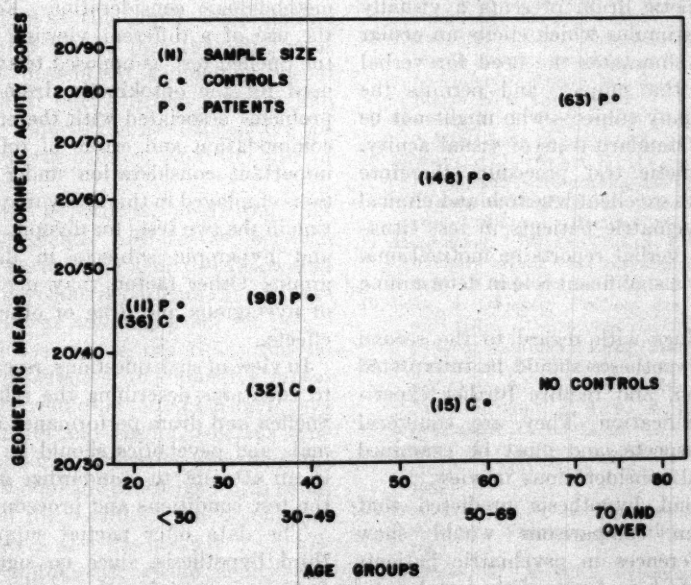

FIG. 2. Mean acuities of controls and psychiatric patients on optokinetic drum by age.

pared. The results are presented in Table 3. Untestable subjects and subjects tested only with glasses were excluded, as they were when the second hypothesis was tested. In addition, seven other subjects whose ages were unknown were excluded from this test. The same adjustment of Snellen scores was made. Geometric means were again computed, using the denominator of the Snellen ratio. The comparisons were made within the following age categories: *under 30*, *30–49*, and *50–69*. The mean acuities for psychiatric subjects and controls are plotted in Figure 2.

Psychiatric patients and controls did not differ significantly in optokinetic acuity in the *under 30* or the *30–49* age groups. The psychiatric group did have significantly poorer mean optokinetic acuity than did the controls in the *50–69* age group. It should be noted, however, that acuity scores of the psychiatric group as a whole became poorer with age over 50, whereas the 15 control subjects in this age group showed no decrement with age. In this analysis, a two-tailed sign test was used, since no hypothesis as to direction of differences was explicitly made. No consistent pattern of differences was evident with respect to the relative acuities of schizophrenics, chronic

brain syndromes and other psychiatric patients. The relationships observed with respect to mean acuities by age and control or diagnostic categories in Snellen scores were the same as the relationships found for the optokinetic tests.

COMMENTS

The results of this study provide clear evidence in support of the first hypothesis. The optokinetic drum presents a visually compelling stimulus which elicits an ocular reflex; this eliminates the need for verbal reports by the subject, and permits the testing of many subjects who might not be testable by standard tests of visual acuity. The optokinetic test procedure therefore represents an excellent research and clinical tool for psychiatric patients in test situations where verbal reports or motivational factors play a significant role in determining test results.

The findings with regard to the second and third hypotheses should be interpreted with caution, and require further experimental clarification. They are equivocal in many respects, and must be examined with several considerations in view.

The second hypothesis predicted that Snellen-drum comparisons would show greater differences in psychiatric patients than in controls in favor of the optokinetic drum acuities. That this hypothesis was not supported was surprising, in view of observations of the erratic nature of the patients' performance on the Snellen test. The halting reading of the letters, abrupt cessation of reading, and other similar performance characteristics often produced the distinct impression that the patients' maximum acuity had not been determined. In the previously cited comparison of the Snellen test with the optokinetic test, Reinecke and Cogan (14) found that the Snellen scores were usually better than the optokinetic scores in normal adult subjects. In addition, Reinecke (13) reported poorer correspondence between the two tests in amblyopic subjects with better acuities. The significantly superior optokinetic acuities found in both groups in the present study differ from these findings. While some differences in acuity estimates obtained by the two tests might have been expected in the patients, the finding of significant differences in acuity by the two test procedures in the controls might be attributed to various methodologic considerations. For example, the use of a different viewing distance in the Snellen test as opposed to the distances used for the optokinetic drum introduced problems associated with the effects of accommodation and errors of refraction. An important consideration under the conditions employed in this study may be a variation in the two tests for myopic, emmetropic and hyperopic subjects in different age groups. Other factors may involve criteria of nystagmus and drug or other treatment effects.

In view of such questions, research leading to equations describing the relationship of Snellen and drum performance in both normals and psychotics should be undertaken in an attempt to standardize and improve the test conditions and procedures.

The data offer partial support for the third hypothesis, since no significant differences in optokinetic acuity were found between psychiatric patients and controls under 50 years of age. The significant differences in optokinetic acuity found between patients and controls over 50 years of age could be due to a bias in sampling. Many of the psychiatric patients over 50 years of age had long histories of institutionalization with concomitant unemployment, whereas all of the 15 controls in this age group were employed in positions requiring visual skills. These controls may have been a highly selected sample, since the ophthalmologic literature indicates that some decrease in acuity over 50 years may be expected in a corresponding population.

SUMMARY

A comparison of visual acuity was made in 47 psychiatric patients and 84 control subjects, using the standard Snellen acuity test and an objective optokinetic nystagmus test procedure. Three specific hypotheses were tested:

The first hypothesis, stating that more psychiatric patients would be testable with the optokinetic test procedure than with the Snellen test, was confirmed. This finding was attributed to the fact that the optokinetic test presents a compelling visual stimulus which elicits ocular reflexes and thus greatly reduces the need for active cooperation by the subject and the use of verbal reports.

The second hypothesis, predicting that significantly greater differences between Snellen and optokinetic acuity scores would be found in psychiatric patients than in normal controls, was not supported.

The third hypothesis, proposing that no significant differences in objective optokinetic acuity would be found between psychiatric patients and normal controls, was partially supported. Possible sampling biases and methodologic problems which could have affected the results of the second and third hypotheses were discussed.

Since the optokinetic visual acuity test offers many advantages, for both research and clinical application, further study involving the relationship of the two acuity tests and the methodologic problems encountered in the present research with psychiatric patients appears to be desirable.

REFERENCES

1. CARMICHAEL, E. A., DIX, M. R. AND HALLPIKE, C. S. Lesions of the cerebral hemispheres and their effects upon optokinetic and caloric nystagmus. Brain, **77**: 345–372, 1954.
2. COGAN, D. G. *Neurology of the Ocular Muscles*. 2nd ed., pp. 197–198. Thomas, Springfield, Illinois, 1956.
3. COUCH, F. H. AND FOX, J. C. Photographic study of ocular movements in mental disease. A.M.A. Arch. Neurol. Psychiat., **31**: 556–578, 1934.
4. DIEFENDORF, A. R. AND DODGE, R. An experimental study of the ocular reactions of the insane. Brain, **31**: 451–489, 1908.
5. DUKE-ELDER, W. S. *Textbook of Ophthalmology*, Vol. 4, pp. 3702–3730. Mosby, St. Louis, 1949.
6. EYSENCK, H. J., GRANGER, G. W. AND BRENGLEMANN, J. C. *Perceptual Processes and Mental Illness*, pp. 23–24. Basic Books, New York, 1957.
7. FOX, J. C. AND HOLMES, G. Optic nystagmus and its value in localization of cerebral lesions. Brain, **49**: 333–371, 1926.
8. FREUD, S. Psychogenic visual disturbance according to psycho-analytical conceptions (1910). In *Collected Papers*, Vol. 2, pp. 105–112. Basic Books, New York, 1959.
9. GIFFORD, E. S. Psychogenic ocular symptoms. A.M.A. Arch. Ophthal., **53**: 318–329, 1955.
10. GORMAN, J. J., COGAN, D. G. AND GELLIS, S. S. An apparatus for grading the visual acuity of infants on the basis of opticokinetic nystagmus. Pediatrics, **19**: 1088–1092, 1957.
11. HENDERSON, J. W. AND CROSBY, E. C. An experimental study of optokinetic responses. A.M.A. Arch. Opthal., **47**: 43–154, 1952.
12. OHM, J. *Objektiv Prüfung der Sehleistungen mit Hilfe der optokinetischen Augenbewegungen*. Ferdinand Enke, Stuttgart, 1953.
13. REINECKE, R. D. Objective and subjective testing of visual acuity in amblyopic patients. Amer. Orthopt. J., **9**: 93–95, 1959.
14. REINECKE, R. D. AND COGAN, D. G. Standardization of objective visual acuity measurements. A.M.A. Arch Ophthal., **60**: 418–421, 1958.
15. SCHLAEGEL, T. F., JR. *Psychosomatic Ophthalmology*. Williams & Wilkins, Baltimore, 1957.
16. SHERMAN, M. A. Optic nystagmus in schizophrenia. Psychiat. Quart., **15**: 797–801, 1941.
17. SMITH, K. U. AND BOJAR, S. The nature of optokinetic reactions in mammals and their significance in the experimental analysis of the neural mechanisms of visual functions. Psychol. Bull., **35**: 193–219, 1938.
18. TICHY, J. Optokinetic nystagmus. Cesk. Neurol., **19**: 192–203, 1956.
19. WALSH, F. B. *Clinical Neuro-Ophthalmology*, 2nd ed., pp. 1166–1179. Williams & Wilkins, Baltimore, 1957.
20. WARKENTIN, J. AND SMITH, K. U. The development of visual acuity in the cat. J. Genet. Psychol., **50**: 371–399, 1937.
21. WEEKLEY, R. D., POTTS, A. M., REBOTON, J. R. AND MAY, R. H. Pigmentary retinopathy in patients receiving high doses of a new phenothiazine. A.M.A. Arch. Ophthal., **64**: 65–76, 1960.

PERCEPTION IN HEMIPLEGIA. IV: BODY SURFACE LOCALIZATION IN HEMIPLEGIC PATIENTS[1]

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To develop appropriate patterns of action, the human organism must be capable of experiencing accurately and of integrating both the stimuli deriving from the environment external to himself and the complex of sensations which derive from his own body.

Earlier investigations (4, 5, 6) reported from our laboratory have been concerned with the manner in which the visual apprehension of certain features of the external environment is modified in hemiplegia. In these earlier studies we found that both right and left hemiplegias were accompanied by systematic alteration in the perception of the visual vertical and horizontal (4), of the visual median plane (5), and in the accuracy of depth perception (6). The findings suggested the possibility that there was a general reorganization of perceptual functioning in hemiplegia, and that our results in the study of visual events would have counterparts in other sensory modalities. As a result, in our most recent studies we have been concerned with exploring additional exteroceptive and proprioceptive functions.

For these reasons, the focus of investigation was expanded in the present study to include an additional aspect of experience: tactual and kinesthetic perception. It was anticipated that these percepts, which contribute fundamentally both to the organization of action and to the development of body-image (15), may also be modified in hemiplegia. If this is indeed the case, then such phenomena as the reduplication of parts (15), denial of the existence of affected limbs (16), and other frequently noted disturbances in body-image organization could arise at least in part from altered perceptions of the body and not merely as manifestations of the psychologic mechanism of denial (16). For these reasons, knowledge of altered perception of "body" and body boundaries appeared to be essential for a fuller understanding of altered functioning in hemiplegia.

SUBJECTS AND METHOD

The ability to locate a touched point on the body surface was studied in hemiplegic and in control patients. Forty-five were subjects from the Bird S. Coler Hospital, of whom 31 had been hospitalized as a result of cerebral vascular accidents. Of these, 15 were right hemiplegics, 16, left hemiplegics. A comparison group was comprised of three healthy staff members and 11 patients suffering from peripheral motor dysfunctions, such as arthritis or fracture, but without any known or apparent central nervous system damage. The clinical status of the hemiplegic subjects is presented in Table 1; neurologic findings are in Table 2.

Localization of touched points on the body surface was studied under two different conditions:

1) The *TK* (Tactual-Kinesthetic) condition, in which localization of a touched point on the body surface was performed without the use of vision (patient blindfolded).

2) The *VTK* (Visual-Tactual-Kinesthetic) condition, in which no restriction of vision was introduced in the localizing task.

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TABLE 1

Clinical Characteristics of the Hemiplegic Patients

#	Age	Duration of Illness	Language and Speech	Initially Dominant Hand	Activities of Daily Living	Ambulation Status
			Right Hemiplegic Group			
1	57	1½ yrs.	Dysarthric	R	Independent	Ambulatory
2	71	3½ yrs.	No disturbance	R dominant, but sometimes used Left	Independent	Wheelchair; can take few steps
3	61	2 CVAs 1 yr. 2 mo. 6 mo.	Mild defective intellectual range			
4	62	2 yrs.	Mild exp. and rec. aphasia	R	Relatively independent: dresses self, combs hair, brushes teeth	Wheelchair (Nurse says, "Could walk if wanted to.")
5	53	1½ yrs.	Severe exp. aphasia	R	Dresses self, puts braces on	Wheelchair; can ambulate on ward
6	62	1½ yrs.	No disturbance	R	Independent	Ambulatory
7	50	1½ yrs.	No disturbance	R	Independent; good functional return in right hand	Ambulatory
8	59	1 yr.	No disturbance	R	Independent; good functional return in right hand	Wheelchair
9	71	8 yrs.	No disturbance	R	Independent	Can walk pushing wheelchair
10	60	1½ yrs.	Mild exp. & rec. aphasia	R	Independent	Ambulatory
11	60	5 yrs.	Dysarthric	R	Dependent for dressing	Wheelchair
12	63	1 yr.	Mild exp. aphasia	R	Independent	Ambulatory
13	52	2 CVAs 10 yrs. 9 yrs.	Exp. severe aphasia	R	Can feed and dress self with left hand	Wheelchair
14	60	2 yrs.	Mild dysarthric	R	Independent	Ambulatory
15	56	1 yr.	Exp. severe aphasia	R	Dresses self, combs hair; cannot tie shoelaces	Wheelchair
		Median Age: 61.2 yrs. Range of Age: 50–71 yrs.				
			Left Hemiplegic Group			
16	58	3 yrs.	No disturbance	R	Dependent in all areas	Wheelchair
17	59	1 yr.	"	R	Independent	Wheelchair; few steps on ward; prognosis good
18	59	4½ yrs.	"	R	Needs help in getting dressed; puts clothes on back to front; can groom herself, but needs detailed supervision; not very alert	Wheelchair
19	64	9 yrs. 12 yrs.	Mild dysarthria	R	Needs help in dressing; cannot shave or button clothes	Wheelchair

TABLE 1—*Continued*

#	Age	Duration of Illness	Language and Speech	Initially Dominant Hand	Activities of Daily Living	Ambulation Status
20	65	3 yrs.	No disturbance	R		Wheelchair, and amb. for short distances
21	60	9 yrs.	"	R	Dresses self but leaves robe open; has to be checked on grooming	Ambulatory, cane and pushes chair
22	77	8 yrs.	"	R	Independent	Chair; walks around on ward
23	66	1 yr.	"	R	Independent	Ambulatory
24	64	4 yrs.	"	R	Dresses with some help, combs own hair, brushes teeth	Wheelchair
25	75	5 yrs.	"	R		Ambulatory
26	80	1 yr.	"	R	Needs help in dressing. Feeds self. Needs supervision	Wheelchair
27	61	2½ yrs.	Dysarthric	R	Independent	Wheelchair
28	79	5 yrs.	No disturbance	R	Independent; good functional return in left hand	Wheelchair
29	59	2 CVAs 5 yrs. ago	"	R	Dresses self; cannot shave or tie shoelaces	Amb., but wheelchair for long distances
30	79	1 yr. 3 mo.	"	R		Wheelchair
31	42	2 CVAs 5 and 2 yrs. ago	"	R	Independent	Ambulatory

Median Age: 63.2 yrs.
Range of Age: 42–80 yrs.

Each subject was tested individually in a quiet, isolated room. All tactual stimuli presented were significantly above threshold. In individual *Ss*, testing under the TK condition was always on a day different from that of the VTK testing. In alternate *Ss*, either the VTK or TK comprised the initial testing series. In all *Ss* the ability to localize touched points on both lower extremities was studied under each condition.

The procedure used was similar to that employed by Renshaw (13) in his developmental studies of body surface localization.

Tactual Kinesthetic Localization: The patient was blindfolded and a cloth containing 1½" squares was placed on the lap directly over the skin. On each thigh a 6 by 4½" rectangle containing 12 squares was defined. A minimum threshold for touch on each leg was determined, then increased by a fixed amount to insure that the patient could feel the stimulus on each trial. The patient held a stylus in his unaffected hand (for non-hemiplegic subjects, right and left hands were alternated), and was instructed as follows: "After I touch you, try to touch exactly the same spot." Twenty-four tactual stimuli were presented (12 on each thigh) in a predetermined random order (Table 3). There was a 30-second interval between each stimulus. The response was scored *correct* if it fell within the same box to which the stimulus had been applied. If it did not fall within the same box, the response distance of displacement from the point of application of the test stimulus was recorded.

Visual-Kinesthetic Localization: The procedure used was the same as that employed for the testing of tactual-kinesthetic local-

zation. In this series, however, an opaque screen was held over the patient's lap while the stimulus was being applied. It was then removed at once, and the patient was instructed to look down at his lap and to touch the previously stimulated point.

RESULTS

Accuracy of Localization: When asked to touch previously stimulated points on the limb surface, hemiplegic patients did so with significantly lower accuracy of localization than did non-hemiplegic subjects. As may be seen in Table 4, when the points to be located are on the affected limb, under both tactual - kinesthetic and visual - tactual-kinesthetic conditions the hemiplegic patients made significantly fewer correct responses than did the non-hemiplegic subjects ($p = .01$). When the unaffected limb was stimulated, localization once again was poorer in the hemiplegic patients than in the non-hemiplegic. However, the group differences obtained were significant only to the .07 level of confidence.

Directionality of Error Displacements: When an error was made in locating a previously stimulated point on the limb surface, displacement from accurate localization could be made in any of a number of directions. The observed displacement may be expressed in terms of a co-ordinate system by means of which a faulty localization may be recorded as being proximal, distal, lateral or medial with respect to the locus of original stimulation. Table 5 shows that the preponderant error made by the hemiplegic group as a whole was a medial one, the locus of stimulation displaced toward the center of the body. Under the tactual-kinesthetic condition, this tendency was largely the contribution of the right hemiplegic patients, who responded with medial errors when either the affected or the intact limbs were stimulated. A similar consistent trend was not present in the left hemiplegic patients under conditions of tactual-kinesthetic testing. As a group, they made an almost equal number of medial and lateral errors when the affected limb was stimulated. The control subjects also failed to show evidence of medial or lateral error preference.

When vision could be utilized by the subject in the localization of the previously touched points, it was found that the medial error became characteristic for all groups. This finding suggested that, when vision was incorporated into the localization task, error displacement tended to be predominantly toward the body midline.

An analysis of the frequency with which errors were made in the proximal distal directions from the points of stimulation is presented in Table 6. This table shows that under tactual-kinesthetic conditions the right hemiplegic group tended to make a preponderance of errors in the proximal direction when the affected limb was tested. The left hemiplegic group, however, exhibited no directional preference when required to localize a touched point on the affected limb. In contrast with these findings, when responses to the stimulation of the unaffected limbs of the hemiplegic groups were studied, a distal directional displacement was preponderant. The control group showed a tendency to make proximal errors on both limbs.

The analysis of error displacements may be summarized as follows:

1) The right hemiplegic patients displaced the perceived locus of touched points systematically toward the body midline and in a proximal direction.

2) The left hemiplegic patients exhibited no preponderant systematic displacement directions, and made errors which were randomly distributed in terms of the four possible directions of displacement.

3) Although the non-hemiplegic subjects made fewer errors, those made resembled those of the right hemiplegic group in that a preponderant error tendency in the proximal direction was discernible. The controls, however, did not exhibit the tendency to displace toward the body midline.

TABLE 2

Cliniconeurologic Findings in the Hemiplegic Patients

#	Etiology	Associated Diseases	Motor Function	Sensory Function	Visual Fields	Abnormal Reflexes
			Right Hemiplegic Group			
1	Cerebral thrombosis	Arteriosclerosis	No active motion in RU; return of function in RL	Intact	Intact	Right Babinski's hyperactive reflexes on right
2	Cerebral thrombosis	Arteriosclerosis	Flaccid paralysis, RU; weakness, RL	Intact	Intact	None
3	Cerebral thrombosis	Hypertension	RU, weakness; RL, weakness	Intact	Intact	Right Babinski's; right Hoffmann's
4	Cerebral thrombosis	Arteriosclerosis, hypertension, diabetes mellitus	Spastic paralysis of RU and RL, with flexion contractures of right hand	Intact	Intact	Right Babinski's
5	Cerebral embolism	Coronary thrombosis	Paretic RU; RL, weakness	Intact	Intact	Hyperactive DTRs on right
6	Cerebral thrombosis	Arteriosclerosis, hypertension	Weakness, right side	Intact tactile functions; temperature loss on right	Intact	Hyperactive DTRs on right
7	Cerebral thrombosis	Arteriosclerosis, ligation of anterior cerebral artery	Slight spasticity, RU; marked spasticity, RL	Reduction of position sense	Intact	Right Babinski's; right ankle clonus
8	Cerebral embolism	ASHD, myocardial infarction	Flaccid paralysis, RU and RL	Intact	Intact	Right Babinski's; slightly hyperactive reflexes on right
9	Cerebral thrombosis	Arteriosclerosis, hypertension	RU, no active movement	Intact	Intact	Right Babinski's; right DTRs slightly hyperactive
10	Cerebral thrombosis	Hypertension	Slight weakness on right	Intact	Intact	Right Babinski's; right ankle clonus; DTRs increased RU
11	Cerebral thrombosis	Arteriosclerosis, diabetes mellitus	RU, paralysis; weakness, RL and LL	Intact?	Marked tubular constriction	Babinski's bilateral
12	Cerebral thrombosis	Arteriosclerosis?	RU, flaccid paralysis; RL, return of active motion in hip and knee	Intact	Intact	Right Babinski's; DTRs hyperactive on right
13	Cerebral thrombosis	Arteriosclerosis	RU, flexion contracture; RL, weakness	Intact	Intact	Right Babinski's
14	Cerebral thrombosis	Hypertension, diabetes mellitus	Flaccid paralysis, RU and RL	Intact	Intact	DTRs hyperactive on right
15	Cerebral thrombosis	Arteriosclerosis, hypertension	RU, flaccid paralysis; RL, weakness	Intact	Intact	Right Babinski's; DTRs hyperactive on right

16	Cerebral thrombosis	Arteriosclerosis, hypertension	LU, spastic paralysis; LL, limited ROM and slight spasticity	Tactual thresholds slightly elevated on LL	Intact	Left Babinski's and left ankle clonus
17	Cerebral thrombosis	Arteriosclerosis	LL, weakness; LU, weakness and sl. spasticity		Intact	Left Babinski's; DTRs hyperactive on left
18	Cerebral thrombosis	Hypertension, diabetes mellitus?	LU and LL, spastic paralysis; tongue deviates to left	Intact	Slight peripheral constriction of temporal fields	Left Babinski's
19	Cerebral embolism	ASHD	Spastic paralysis of LU; weakness of LL	Intact	Slight restriction, left temp. fields	Left Babinski's; left Hoffmann's; hyperactive DTRs on left
20	Cerebral thrombosis	ASHD	Flexion contractures of left elbow; LL, weakness	Intact	Intact	Hyperactive DTRs on left
21	Cerebral thrombosis	Arteriosclerosis	LU and LL, weakness	Intact	Intact	Babinski's bilateral; hyperactive DTRs on left
22	Insufficient neurologic information				Intact	
23	Lues, granuloma	Lues	Flexion contracture, LU; LL, weakness	Intact	Intact	Left Babinski's; hyperactive DTRs on left
24	Cerebral thrombosis	Hypertension	LU, moderate spasticity; left foot drop; active motion with slight weakness, LL	Slight loss fine touch on left	Slight peripheral constriction, left visual field	Left Babinski's; hyperactive L, DTRs
25	Cerebral thrombosis, possible trauma	Arteriosclerosis; rheumatoid arthritis	LU, paralysis; LL, weakness	Intact	Intact	Left Babinski's; hyperactive DTRs on left
26	Cerebral thrombosis	Arteriosclerosis, hypertension	LU, spastic paralysis; LL, weakness	Diminished superficial and deep sensation on left	Unco-operative	Hyperactive DTRs on left
27	Cerebral thrombosis	Hypertension, diabetes mellitus	LU, spastic paralysis; LL, active motion at hip	Intact	Intact	Left Babinski's; left Hoffmann's
28	Cerebral thrombosis	Arteriosclerosis, hypertension, arthritis both knees	LL, slight weakness	Intact	Intact	
29	Insufficient neurologic information				Intact	
30	Insufficient neurologic information				Intact	
31	Cerebral thrombosis	Hypertension, diabetes mellitus, removal spinal tumor in 1948	LUE, good motor power; LL, weakness quadriceps; minimal function, left ankle	Questionable position sense loss on left	Intact	Left Babinski's; left Hoffmann's; hyperactive DTRs on left

TABLE 3

Order of Points Stimulated on Each Thigh

12	6	8
3	1	10
9	7	4
5	11	2

findings. The data on body surface localization were therefore analyzed to determine whether they were systematically related either to clinical functional status or to specific localizing neurologic signs. No significant relation was found to exist between the magnitude or the direction of error displacement, and such characteristics as duration of illness, presence of pathologic reflexes, visual field disturbance, degree of motor dysfunction, or presumptive etiology.

TABLE 4

Average Number of Correct Localizations of Touched Points

	N	TK		VTK	
		Affected*	Unaffected	Affected	Unaffected
Right Hemi.	15	4.66 ± 2.28	5.53 ± 3.20	4.73 ± 1.80	5.87 ± 2.74
Left Hemi.	16	3.20 ± 2.69	6.40 ± 2.25	4.47 ± 2.87	7.13 ± 2.23
Non-Hemi.	14	6.43 ± 2.87	6.85 ± 2.71	7.85 ± 4.15	6.78 ± 2.92

* For Non-Hemi. Affected = Right
Unaffected = Left

TABLE 5

Frequency of Medial and Lateral Error Displacement

	N	TK				VTK			
		Affected*		Unaffected		Affected		Unaffected	
		M	L	M	L	M	L	M	L
RH	15	41	22	40	18	59	14	30	22
LH	16	42	44	27	17	47	18	16	7
NH	14	28	20	30	43	15	3	39	6

* For Non-Hemi. Affected = Right
Unaffected = Left

TABLE 6

Frequency of Proximal and Distal Error Displacement

	N	TK				VTK			
		Affected*		Unaffected		Affected		Unaffected	
		P	D	P	D	P	D	P	D
RH	15	49	26	24	35	20	51	12	47
LH	16	45	45	20	39	26	53	13	44
NH	14	38	12	30	17	39	35	23	19

* For Non-Hemi. Affected = Right
Unaffected = Left

As may be seen in Tables 1 and 2, the hemiplegic patients differed from one another with respect both to their general functional capacities and to their neurologic

Since it was possible that a higher threshold for tactual stimuli in the affected limb could have been the reason for the fewer correct responses made by the hemiplegic patients, tactual pressure thresholds were determined and the data examined to determine whether the obtained differences in correct responses on the affected and intact limbs of the hemiplegic subjects could have occurred as a function of a raised threshold for touch. However, this interpretation was not substantiated by the data on tactual sensitivity. Although some patients who had high tactual thresholds localized less accurately than did the average on the affected limb, this finding was not at all consistent. Interestingly, patients who had equal tactile thresholds on both the affected and the unaffected legs still exhibited increased error tendency on the affected limb. There thus appeared to be no simple or direct relationship between the elevation of tactual threshold and the patient's ability to localize previously touched points on the body surface.

QUALITATIVE PHENOMENA

Since none of the groups studied was truly homogeneous, it is of value to report qualitative features of individual functioning as well as overall group results. The hemiplegic patients presented a number of disturbances in sensation and action not manifested by *Ss* in the control comparison group. These dysfunctions included:

1) Translocation.
2) Localization of the touched points off the body surface.
3) Sensation in the absence of applied stimulation.
4) Duplicated sensation in body parts other than the limb stimulated.
5) Diffuse sensation.
6) Persistent after-sensation.
7) Perseverative responding.

In some hemiplegic *Ss*, sensation of a stimulus applied to the affected limb was translocated to an equivalent portion of the intact extremity. The phenomenon of translocation may be viewed as an exaggeration of the general tendency to displace locus of stimulation toward the intact side. Translocations occurred predominantly under the tactual-kinesthetic test condition, and appeared only infrequently when vision was utilized. However, one patient who during examination asked, "Where is my bad arm?," and could locate it only on a visual basis, persisted in the translocation of stimulated points from the affected to the nonaffected limb in all 24 trials under both tactual-kinesthetic and visual-tactual-kinesthetic conditions of testing.

Several of the hemiplegic patients showed a tendency to locate the touched point entirely off the body surface. When touched on the affected limb in the tactual-kinesthetic condition, they reached laterally and touched points on the covering cloth which were frequently as much as 2–6″ (4–13 cm.) beyond the lateral border of the touched extremity. Both right and left hemiplegic patients made "off-body-surface" localizations. No errors of this type were made under the visual-tactual-kinesthetic conditions of testing.

A number of inappropriate paresthetic experiences were reported by the hemiplegic subjects. When a point on the affected limb surface was touched, several *Ss* stated that they felt diffuse stimulation of the limb in addition to a punctiform stimulus. At times they also reported that the overlying cloth felt as though it were slipping, or were being moved over the intact extremity. In some patients the punctiform sensation applied to the affected limb was felt as being duplicated on the intact lower extremity, or on either arm, or on both an arm and the intact lower limb. In one patient, duplication of sensation was experienced on the homolateral flank and abdomen. Additional distortions of localization involved the reported continuance of the sensation for a prolonged period after the removal of the stimulus, and intermittent experiences of restimulation when no new stimulus was being applied. This latter disturbance resulted in the patient's making repeated perseverative localizing responses (often as many as five to seven) to a single application of a stimulus. That this behavior was not a manifestation of the general perseverative tendency found in some brain-injured patients is indicated by the complete absence of such repetitive responding when the intact rather than the affected limb was stimulated. Further, when vision was available, although the patient reported the experience of repeated "phantom" stimulation, the visual knowledge that no new stimulus was being applied resulted in the effective elimination of the tendency to repeat localizing responses.

Two general features of tactual perception found in the hemiplegic patients were rigidity and uncertainty. When a given hemiplegic patient did point to more than one spot, he wavered and functioned vaguely, and appeared to be unsure of the locus stimulated, rather than engaged in an attempt to correct the original inaccurate response. In contrast, when the controls attempted

to correct initially inaccurate responses, they exhibited definite choice behavior, and said, "This is not the spot you touched—it's above or below it—it just doesn't feel like the same spot that you touched after I touch it." No hemiplegic patient made this type of explicit discriminatory statement.

DISCUSSION

This discussion will attempt to accomplish two objectives: certain problems deriving from the specific data on tactual localization will be considered; and the data of the present study will be viewed, together with our earlier findings on visual perceptual alteration (4, 5, 6), as one aspect of the more general problem of body-scheme or body-image.

In our study, the degree of disability in tactual localization present in the right hemiplegics was notably less marked than in the left hemiplegics. This apparent superiority in perceptual functioning on the part of the right hemiplegic patients had been previously noted in our studies of visual perception. Several tentative explanations for these differences can be offered, prominent among them the view (8, 9) that damage to the subdominant hemisphere results in greater degrees of denial and perceptual distortion. However, the simplest explanation supported by the data is that a factor of social selection operates to produce the hospitalization and institutionalization of less severely impaired right hemiplegics. In our series of patients it appeared that the disturbances of language function accompanying dominant hemisphere damage had resulted in the institutionalization of more generally intact right than left hemiplegics. This view is given support by the fact that the general intellectual level of our patients as measured by the Wechsler-Bellevue Intelligence Scale (Form 1) was higher in our right than in our left hemiplegics.

Our data provide some evidence of the inter-relation of the sensory systems in perception. It is of interest that the use of vision served to modify the patterns of localization of tactual stimulation. Thus patients who, when blindfolded, tended to localize touched points off the body surface, with one exception succeeded in making on-body localizations when vision was unimpeded. Further, the use of vision resulted, in general, both in improved accuracy of localization and in an increased tendency to displace locus of stimulation toward the center of the body. It appeared that, with the development of increased difficulty in tactual localization following cerebral damage, patients manifest increased dependence upon vision for tactual localization. This appears to be an exaggeration of the normal developmental tendency to become increasingly dependent upon vision in tactual localization, demonstrated by Renshaw some years ago (14).

The data obtained in the present report, taken together with our earlier findings (4, 5, 6), indicate that, in addition to their motor losses, patients with hemiplegia sustain significant disturbances in the ability to perceive both the visual outer world and tactual events occurring at the body surface. These modifications in function, almost by definition, represent a reorganization of body schema. Since such a plethora of behaviors has come to be subsumed under this general category, it is desirable to specify our use of the construct.

Historically, the idea of body schema originated in some tentative formulations of Wernicke (9), who believed that certain systematic disturbances in the perception of one's body and of the outside world could best be understood in terms of a "somatopsyche" related to self, and an "allopsyche" that had reference to the external world. The concept was formalized by Pick (12), Head and Holmes (10) and Babinski (2), and by the time of the First World War was well established as a neurologic construct. In its clearest meaning, the body-image or, more appropriately, body-schema,

which has no implications of awareness or consciousness, represents a functional organization of the nervous system that has been evolved in the life course of the person. It is the learned integration of sensations which, in their organization and structuring, determine the orientation of a person to his own body and organs, as well as the relation of his body to the world about him.

Two aspects of the body-schema concept must be recognized with special clarity. First, it is based upon the totality of inputs into the nervous system—the whole of the person's sensations. Second, it is a learned organization of sensory integrations, subject both to the laws of learning and to the variety of disturbances in learned patterns that may result from pathologic changes in the structure or activity of the nervous system. In brief, then, body-schema may be said to be that learned organization of sensory inputs which determines the individual's awareness of body and the orientation of his body to the world external to him.

A host of implications stems from this formulation of the construct. In the first place, since the development of the body-schema is based upon sensory inputs, peculiarities in afferent function ranging from loss of sense modalities, as in blindness or deafness, to more trivial peculiarities in receptivity can and do modify the nature of the schema that is elaborated (15). Loss of sensory input through pathologies of receptor organs, particularly of vision, touch, somesthesia and proprioception, can be expected to result in disturbances of body and external world orientation. Further, it can be expected that drugs (*e.g.*, barbiturates or lysergic acid) which modify either the character of sensory activity (1) or central integration will produce alterations in body-schema. More generally, experimental situations such as those devised by Lilly (11) and Bexton (3), which deprive the person of the usual patterns of input, should be expected to yield profound and disturbing alterations in the individual's orientation to his body and his world, even to the extent of hallucinatory experiences.

If attention is for a moment directed away from sensory input toward the central nervous system upon which such afferents impinge, it is clear that peculiarities of central activity may also result in bizarre integrations of the inputs, and thereby interfere with the functionally adequate elaboration of body-schema. In our patients with hemiplegia, and especially in those with damage to the subdominant hemisphere, it appears that changes both in afferent input and in central integration have resulted in a profound modification in body-schema. It is probable that this contributes at least as substantially to disability in hemiplegia as does the motor deficit.

SUMMARY

The ability to localize touched points on the body surface was studied in a series of hemiplegic patients and control subjects. Significant impairment was noted in the hemiplegics, most markedly in subjects with subdominant hemisphere damage. The data were discussed as an aspect of the general problem of body-schema reorganization in cerebral injury.

REFERENCES

1. Apter, J. T. and Pfeiffer, C. C. The effect of hallucinogenic drugs LSD-25 and mescaline on the electroretinogram. Ann. N. Y. Acad. Sci., **66**: 508–514, 1957.
2. Babinski, J. Contribution a l'etude des troubles mentaux dans l'hemiplegie organique cerebrale (anosognosie). Rev. Neurol., **27**: 845–847, 1914.
3. Bexton, W. H., Heron, W. and Scott, T. H. Effects of decreased variation in the sensory environment. Canad. J. Psychol., **8**: 70–76, 1954.
4. Birch, H. G., Proctor, F., Bortner, M. and Lowenthal, M. Perception in hemiplegia: I. The judgment of vertical and horizontal by hemiplegic patients. Arch. Phys. Med., **41**: 19–27, 1960.
5. Birch, H. G., Proctor, F., Bortner, M. and Lowenthal, M. Perception in hemiplegia: II. Judgment of the median plane. Arch. Phys. Med., **41**: 71–75, 1960.
6. Birch, H. G., Proctor, F., Bortner, M. and Lowenthal, M. Perception in hemiplegia:

III. The judgment of relative distance in the visual field. Arch. Phys. Med. In press.

7. BUCY, M. *Les troubles de la sensibilite dans l'hemiplegia d'origine cerebrale.* Paris Theses: No. 383. University of Paris, 1902.
8. CRITCHLEY, M. *The Parietal Lobes.* Arnold, London, 1953.
9. GERSTMANN, J. Psychological and phenomenological aspects of disorders of the body image. J. Nerv. Ment. Dis., **126:** 499–512, 1958.
10. HEAD, H. AND HOLMES, G. Sensory disturbances from cerebral lesions. Brain, **34:** 102–254, 1911.
11. LILLY, J. C. Mental effects of reduction of ordinary levels of physical stimuli on intact healthy persons. Psychiat. Res. Rep. Amer. Psychiat. Ass., **5:** 1–28, 1956.
12. PICK, A. Über Störungen der Orientierung am eigenen Körper. Arbeit. deutsch. psychiat. Univ. Klin. Prague. Berlin, 1908.
13. RENSHAW, S. The errors of cutaneous localization and the effect of practice on the localizing movement in children and adults. J. Genet. Psychol., **38:** 223–238, 1930.
14. RENSHAW, S., WHERRY, R. J. AND NEWLIN, J. C. Cutaneous localization in congenitally blind versus seeing children and adults. J. Genet. Psychol., **38:** 239–248, 1930.
15. SCHILDER, P. *The Image and Appearance of the Human Body.* International Universities Press, New York, 1950.
16. WEINSTEIN, E. A. AND KAHN, R. L. *Denial of Illness.* Thomas, Springfield, Illinois, 1955.

SIDE-EFFECTS, RESISTANCE AND DOSAGE DEVIATIONS IN PSYCHIATRIC OUTPATIENTS TREATED WITH TRANQUILIZERS[1]

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There are three ways in which any outpatient's reaction to medication may attenuate the benefits to be desired from his treatment: 1) he may develop side-effects or more serious complications; 2) he may harbor an attitude of resistance to medication which can vitiate the subjective aspects of benefit, even if he is complying with the prescribed dosage; 3) he may, with or without an attitude of resistance, fail to take the medicine in the prescribed amount or at the specified frequency. Of these three reactions, only side-effects are treated extensively in the literature.

In typical papers on side-effects in clinical psychopharmacology (2, 3), it is assumed for the most part that they are pharmacologic consequences of the medication. Only occasionally have psychodynamic factors in side-effects received serious clinical and theoretical attention (7). Likewise rare are experimental studies in which side-effects from active medications are objectively evaluated by comparison with side-effects from placebos (9). To attribute the harmful effects of a medication to its direct pharmacologic action, without appropriate control observations such as with a placebo, is as fallacious as to claim therapeutic benefits without similar controls which show less improvement. Such an assumption is especially dubious in outpatient treatment, where dosage levels for tranquilizers and other psychiatric drugs are usually low enough to preclude the more serious complications, *e.g.*, Parkinsonism and agranulocytosis. The authentic "pharmacologic profile" of the drug is often unobservable except at higher or maximum dosage levels. At lower dosages, like those used in this study, the nature and mechanisms of "side-effects" are an open scientific question.

SOURCE OF DATA

Data for this report were collected in a Veterans Administration co-operative study of adjunct chemotherapy in 22 mental hygiene clinics[3] (4). The subjects in that project were 180 adult male psychiatric outpatients. To be included in the co-operative study, patients had to be younger than 50, and acceptable for individual psychotherapy once weekly, which was the primary treatment for all patients. Their condition had to be such that they could safely be without tranquilizers for three weeks before starting treatment. They must have had no history of central nervous system pathology or seizures, and no psychiatric hospitalization during the preceding three months. Finally, they were required to be without alcohol addiction or any active illness that might be aggravated by any of the drugs used. With these limitations, patients were accepted in

[1] A preliminary report of this study was co-authored and read by Louis A. Cibelli, M.D., at the Fifth Annual Research Conference, VA Cooperative Chemotherapy Studies in Psychiatry and Research Approaches to Mental Illness, Cincinnati, Ohio, June, 1960 (5).

The medications used in the study were furnished to the Veterans Administration through the courtesy of Wallace Laboratories and Smith, Kline & French Laboratories.

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[3] We acknowledge the co-operation of the staffs of Mental Hygiene Clinics in the following cities: Atlanta, Baltimore, Boston, Buffalo, Chicago, Cleveland, Denver, Des Moines, Hartford, Los Angeles, Lowell, New Orleans, New York, Phoenix, Philadelphia, Pittsburgh, Roanoke, St. Louis, St. Paul, St. Petersburg, San Francisco and Syracuse.

TABLE 1

*Relation Between Type of Medication Received and Number of Side-Effects**

	0 Side-Effects	1 or 2 Side-Effects	3 or More Side-Effects	All Patients	
				N	Per Cent
Per Cent of Patients Receiving:					
Chlorpromazine	28	34	38	32	22
Meprobamate	45	26	29	38	27
Phenobarbital	47	30	23	30	21
Placebo	42	29	29	42	30
All Patients: Number	58	42	42	142	
Per Cent	40	30	30		100

* Not significant ($p < .80$).

order as they applied for treatment, and each was randomly assigned to one of the following treatment groups, in which he remained until medication was withdrawn at the end of eight weeks:

Treatment	No. of Patients
Psychotherapy plus chlorpromazine (100 mg. daily)[4]	32
Psychotherapy plus meprobamate (1600 mg. daily)[4]	38
Psychotherapy plus phenobarbital (2 gr. daily)	30
Psychotherapy plus inert placebo (lactose and magnesium stearate)	42
Psychotherapy only	38
Total	180

The subjects of this study were the 142 patients who received medication, *i.e.*, those in the first four rows above. Medication four times a day was provided double-blind by a prescribing physician, usually a psychiatrist, who saw the patient once a week just after his weekly psychotherapy session. As far as possible, patients were treated as any other clinic patients, and were not told they were in a research study. The prescribing physicians never served as psychotherapists for any patients whose medication they prescribed.

[4] For the first week, the two tranquilizers were administered at half strength.

Side-effects were recorded by the physicians on a weekly checklist containing 26 items, with extra spaces for write-ins. In evaluating side-effects, physicians were free to make special examinations or laboratory tests at their discretion. In addition to checking for side-effects, they were asked from week to week to answer or rate various items covering the patient's attitude and response to medication.

SIDE-EFFECTS: INCIDENCE AND INTERCORRELATIONS

In the data analysis, each side-effect was counted *present* if it was observed at any time from the second through the ninth (final) visit, and *absent* if not observed at all.

Table 1 shows that 40 per cent of the patients in the study had no side-effects at any time during their study period, 30 per cent had one or two, and 30 per cent had three or more; the corresponding percentages for each of the four medication groups are also given. The negative finding of no significant differences among the four groups[5] suggests that, in the total patient group, variation in number of side-effects is a function of some factor(s) other than, or in addition to, medication alone. In particular, the patients on placebo were about as prone to develop side-effects as were those on any of the active medications. This is consistent with Wolf and Pinsky's remark: "Since widespread and profound tissue responses may occur following placebo administration, it is to be expected that certain unfavorable changes may occur that would be classified as toxic reactions" (9).

In only two cases (not included in Table 1) was it necessary to discontinue medication and remove patients from the study because of side-effects that were judged to be unduly severe. In one case the side-effect was a skin rash; in the other, depression. Both patients had been receiving phenobarbital.

[5] Except where otherwise indicated, all significance tests are based on chi-square, using the raw frequencies from which the reported percentages are derived.

TABLE 2

Median, Minimum and Maximum Number of Side-Effects in Each Medication Group

	Median	Minimum (for any Pt.)	Maximum (for any Pt.)
Chlorpromazine	1.70	0	14
Meprobamate	0.90	0	8
Phenobarbital	0.75	0	7
Placebo	1.10	0	13
All Patients	1.15	0	14

Table 2 provides additional descriptive information on the side-effects distributions. As to the maximum number per patient, the placebo is again well within the range of the active medications.

In merely counting side-effects impartially, we may be overlooking critical "qualitative" differences. A specific side-effect experienced by a patient on chlorpromazine may have been quite different from some other side-effect suffered by a patient on placebo. Table 3 indicates, however, that in general such was not the case. This table lists the most common side-effects in order of decreasing frequency in the total patient group.[6] Only the first four side-effects occurred with sufficient frequency to permit tests of significance; the last column shows the p values for these results. None reached the .05 level; there is thus no conclusive indication that their distribution among the four medication groups is other than a chance one. Tension, however, does tend to appear more frequently in patients on chlorpromazine and meprobamate ($p < .10$), and weakness or fatigue in those on chlorpromazine and phenobarbital ($p < .10$). For the list as a whole, most of the common reactions fall into two categories: disturbances of activity level, and dysfunctions of the autonomic nervous system. There are some striking similarities between the reactions in Table 3 and those cited in Beecher's survey (1) as "toxic and other subjective side-effects of placebos."

Do single side-effects tend to occur independently of one another, or is there a "side-effect proneness" similar to accident proneness? The latter possibility is clearly suggested by the data given in Table 4. In this analysis we used only the first eight side-effects in Table 3—*i.e.*, those occurring in ten per cent or more of the total group of

TABLE 3

Relation Between Type of Medication Received and Incidence of Most Common Side-Effects

Side-Effect "Present"	Per Cent of Patients Receiving: Chlorpromazine	Meprobamate	Phenobarbital	Placebo	All Patients N	All Patients Per Cent	p
Drowsiness, lethargy	31*	45	40	36	54	38	.70
Tension	31	24	7	17	28	20	.10
Restlessness, excitement	22	16	7	24	25	18	.30
Weakness, fatigue	28	16	20	5	23	16	.10
Vivid or frequent dreams	25	8	10	5	15	11	
Nausea, GI discomfort	16	8	3	14	15	11	
Dryness of mouth or throat	19	3	7	12	14	10	
Headache, "hang-over" effect	9	8	3	17	14	10	
Constipation	6	11	3	7	10	7	
Rapid pulse	12	5	0	2	7	5	
Blurred vision	12	0	3	2	5	4	
Tremor	16	0	0	0	5	4	

* The percentages are read as follows: 31 per cent of those patients who received chlorpromazine reported drowsiness or lethargy as a side-effect, and so on.

[6] Not listed are the following 33 side-effects, each of which had an incidence of less than ten per cent in any of the four medication groups, and most of which occurred in only one per cent (one or two cases) of the total group of 142 patients: euphoria, slowed muscular reactions, nasal congestion, dizziness, excessive salivation, dermatitis, aches and pains, paresthesia, muscle rigidity, impotence or reduced potency, gustatism, anorexia, impaired associated movements, facial rigidity, fainting, feverishness, photosensitivity, fluid retention, swelling, faintness, impaired thinking or memory, stiff neck, tinnitus, hunger, depersonalization, polyuria, heartburn, depression, diarrhea, hyperhidrosis, psychic regression, slowing of speech, and irritability.

TABLE 4

Fourfold Point Correlations Among the Eight Most Frequent Side-Effects

	Drowsiness, Lethargy	Restlessness, Excitement	Tension	Vivid or Frequent Dreams	Weakness, Fatigue	Nausea, GI Discomfort	Dryness of Mouth or Throat
Restlessness, excitement	.25						
Tension	.27	.58					
Vivid or frequent dreams	.18*	.44	.29				
Weakness, fatigue	.35	.25	.22	.33			
Nausea, GI discomfort	.33	.38	.29	.23	.09		
Dryness of mouth or throat	.18*	.34	.32	.18*	.18*	.12	
Headache, "hang-over" effect	.23	.34	.19*	.26	.11	.19*	.13

* Significant at the .05 level. Correlations higher than .19 are significant at the .01 or .001 level; those lower than .18 fall below the .10 level of significance.

142 patients. These, when counted by number of patients manifesting each, covered 62 per cent of all side-effects reported. Of the 28 intercorrelations shown, only four failed to reach a significance level of .05 or higher:

Nausea, GI discomfort—Weakness, Fatigue, $\phi = .09$
Nausea, GI discomfort—Dryness of mouth or throat, $\phi = .12$
Dryness of mouth or throat—Headache, "hang-over" effect, $\phi = .13$
Headache, "hang-over" effect—Weakness, fatigue, $\phi = .11$

The median of all correlations in Table 4 is .25. The numerous significant correlations suggest that the separate side-effects may be, at least in part, syndromic components of a unitary tendency to develop adverse reactions to any medication in this study, including placebo.

RELATIONS BETWEEN SIDE-EFFECTS AND PRESENTING COMPLAINTS

Since side-effects were largely unrelated to type of medication, it was possible that they were merely symptoms present before effects by the patients or by their physicians. A partial check on this hypothesis was afforded by seven complaint items in a pencil-and-paper questionnaire which all patients took shortly before starting medication. These items were worded identically or quite similarly to the following seven common side-effects: drowsiness, lethargy; tension; restlessness, excitement; weakness, fatigue; vivid or frequent dreams; nausea, GI discomfort; and headache, "hang-over" effect. On a four-point scale ranging from "Not at all" to "Extremely," the patients were asked to what extent they had been troubled by these complaints during the preceding few weeks.

The patients' pre-treatment questionnaire responses to each of these items were dichotomized at the point nearest the median scale value of each response and tested against the presence or absence of the corresponding side-effects as recorded by the physicians during treatment. Only one of these seven tests, that for tension, showed a significant relationship ($p < .01$) in the expected direction—*i.e.*, pre-treatment complaint positively associated with corresponding side-effect. For the remaining six items the p values did not approach significance (.50 to .98). In four of these six the relationship was in the expected direction, while the two other side-effects for which the expected relationship did not hold even on a chance level were 1) restlessness, excitement, and 2) weakness, fatigue. Altogether, these results lend little support to the possibility that specific presenting symptoms in this study may have been confounded with side-effects.

SIDE-EFFECTS AS A FUNCTION OF PRE-TREATMENT ATTITUDE TO MEDICATION

After seeing each patient for the first time and writing the double-blind prescription, the physician rated the patient's attitude to

medication on a five-point scale. The scale values and numbers of patients so assigned were:

1. Very eager	17
2. Somewhat eager	52
3. Neither eager nor reluctant	64
4. Somewhat reluctant	9
5. Very reluctant	0

The absence of subjects in the "very reluctant" group is no doubt attributable to the impossibility of persuading such patients to accept drug treatment. Because the distribution was truncated at this end, the 73 patients in groups 3 and 4 were pooled under the heading "not eager," thus leaving three groups for analysis of the attitude variable.

Table 5 shows that the relation between attitude to medication and number of side-effects is neither random nor linear. Assuming that the relation is stable and could be replicated, we can note two patterns for predicting side-effects from attitude:

1) A patient rated "somewhat eager" is most likely to have no side-effects.

2) A patient rated "very eager" is most likely to have one or two side-effects. (The number of side-effects for patients in the "not eager" category shows no trend distinguishable from that of the total patient group.)

TABLE 5

*Relation Between Patients' Attitude to Medication and Number of Side-Effects**

Attitude to Medication	0 Side-Effects	1 or 2 Side-Effects	3 or More Side-Effects	All Patients	
				N	Per Cent
Per Cent of Patients Rated:					
Very eager	23	59	18	17	12
Somewhat eager	52	21	27	52	37
Not eager	37	29	34	73	51
All Patients: Number	58	42	42	142	
Per Cent	40	30	30		100

* Significant at .05 level.

EMOTIONAL RESISTANCE TO MEDICATION AND DEVIATION FROM PRESCRIBED DOSAGE

Each week after the initial visit the physician was asked, in writing, several yes-or-no questions, including the following two:

1) "Has the patient manifested any serious resistance to medication?"

2) "Has the patient deviated from prescribed dosage amount or frequency?"

Answers were to cover the entire period since the previous visit. The data from the first question can be considered to reflect the emotional level of reactions to medication (fear, aversion, impatience, disappointment, and so on), while the second question reflects the behavioral level—both supplementing the data on the body-function level as charted by side-effects. The following samplings from the physicians' weekly comments on resistances and deviations indicate some of the personal meanings of these reactions to the patients who manifested them.

Resistance without deviation: Patients who resisted medication but did not during the same week deviate from the prescribed dosage were typically subjective or evaluative in their outlook.

> (Case 1—Phenobarbital) "Concerned that he may become too dependent on medication."
>
> (Case 2—Phenobarbital) "'Pills doing no good.' However, states that he will continue as prescribed."

In some instances the patient's show of resistance was associated with somatic complaints which the physician did not always consider to be side-effects of the drug:

> (Case 3—Chlorpromazine) "Veteran tempted to discard medicine after second failure at intercourse. Talked with him and he agreed to continue."

Deviation without resistance: Very rarely did any patient deviate from the prescription in the direction of increasing his dosage, *e.g.*, to five capsules daily instead of four. Virtually all deviations consisted of skipping one or more doses or days. The physicians' failure to report emotional resistance in many of these cases may have been a function of the brevity and the relative superficiality of their contacts with the patients.

At any rate, the typical deviation without resistance was described in terms which might be summarized as "ostensibly involuntary." Such deviations usually occurred in conjunction with some irregularity or change in the patient's daily routine which did not facilitate or synchronize with observance of the medication schedule calling for one capsule after each meal and one at bedtime.

(Case 1—Chlorpromazine) "Learn today that patient only eats two meals a day, so has taken three capsules instead of four."

(Case 2—Placebo) "Missed an occasional dose due to change of shifts on job."

(Case 3—Meprobamate) "Taking medication regularly until last day or so. Visited brother-in-law and forgot to take medication with him."

(Case 4—Meprobamate) "Since he missed last week's appointment, has had no medication for about one week."

Other instances of deviation were recorded by the physician as simply forgetting to take the capsules at the scheduled time. The next category includes those cases of forgetfulness where the physician judged that serious resistance to medication was involved during the same weekly period in which the patient forgot.

Resistance and deviation concurrent: The first case illustrates forgetting interpreted as resistance:

(Case 1—Meprobamate) "Apparently missed two or three doses this week. Blames it on 'forgetting.' Wife had them in her purse, etc. Resistance?"

Missed appointments also were sometimes associated with resistance to medication:

(Case 2—Phenobarbital) "Patient failed to keep appointment for March 13. [In the initial contact, patients were advised to telephone for make-ups for all missed appointments, so as not to deplete their medication.] Said he could not reach a telephone until 4:00 p.m. Also, didn't know telephone number of clinic. No medication for four or five days."

Both deviation and resistance could occur either in anticipation of side-effects or in retrospective reaction to them. In case 3 below, illustrating anticipation, deviation and resistance were reported at the end of the first week, and no side-effects were recorded for that period:

(Case 3—Phenobarbital) "Patient has taken medication only after dinner and at night. Afraid he will fall asleep at work."

(Case 4—Placebo) This patient, although he was receiving placebo, was reported to have "stopped taking medication last Friday because of dizzy feelings and nausea." Later, the record read: "Medication made him feel worse. Stopped last week."

In tabulating for statistical analysis the physicians' answers to the yes-or-no questions on resistance and deviation, we used the same notation as for side-effects: "present" whenever one or more "yes" answers appeared at any time. (Flagrant deviants were eliminated from the study; they will be discussed later.)

Resistance and deviation were significantly positively correlated ($\phi = .32$, $p < .001$). Of the 21 patients who were resistors, 90 per cent were also dosage deviators, whereas of the 74 patients who were dosage deviators, only 26 per cent were also resistors. In other words, an emotionally resistant patient was almost always a dosage deviator at some time during his eight weeks of treatment, but deviators greatly outnumbered resistors, and included many patients who were not judged as showing emotional resistance to medication in any of their visits with the prescribing physicians. As we have already suggested, this picture might have been different if the patient's psychotherapist had been the person who dispensed the medicine and made the observations on dosage deviation and resistance.

Table 6 shows that, like side-effects, neither resistance nor deviation is related to the type of medication received by the patient ($p < .20$ and $p < .80$, respectively). On the other hand, while attitude to medication has been shown to be antecedently related to number of side-effects (Table 5), it is not so related either to resistance or to deviation (Table 6: $p < .80$ and $p < .30$, respectively). This surprising finding suggests

TABLE 6

Resistance to Medication and Dosage Deviation as Functions of Type of Medication, Attitude to Medication, and Number of Side-Effects

	Resistance to Medication				Dosage Deviation				All Patients	
	Present	Absent	Total	p	Present	Absent	Total	p	N	Per Cent
Per Cent of Patients Receiving:										
Chlorpromazine	19	81	100		47	53	100		32	22
Meprobamate	8	92	100		58	42	100		38	27
Phenobarbital	27	73	100		47	53	100		30	21
Placebo	10	90	100	.20	55	45	100	.80	42	30
Per Cent of Patients Rated:										
Very eager	18	82	100		65	35	100		17	12
Somewhat eager	12	88	100		44	56	100		52	37
Not eager	16	84	100	.80	55	45	100	.30	73	51
Per Cent of Patients Having:										
3 or more side-effects	21	79	100		52	48	100		42	30
1 or 2 side-effects	17	83	100		67	33	100		42	30
0 side-effects	9	91	100	.10	41	59	100	.05	58	40
All Patients: Number	21	121			74	68			142	
Per Cent	15	85			52	48				100

that in the patients' attitudes to medication, as manifested to and rated by the prescribing physicians, immediate situational and defensive factors may have predominated.

Table 6 also presents the relation of side-effects to resistance and to deviation. (Side-effects cannot, however, be considered to have *preceded* resistance or deviation, since all three variables were tabulated simply on the basis of their occurrence or non-occurrence at any time during the eight-week medication period; any one of the three may have preceded either of the others in a given patient.) For side-effects and resistance, the relation approaches significance ($p < .10$), is linear, and is in the expected direction, *i.e.*, more side-effects associated with more frequent resistance, and conversely. For side-effects and deviation, the relation is less consistent but significant ($p < .05$), and again generally in the expected direction. Patients with no side-effects are much less likely to be deviators (41 per cent) than are patients with one or two side-effects (67 per cent). Among patients with three or more side-effects, however, the deviation rate (52 per cent) is not different from the deviation rate for the total patient group.

Thus the evidence is that, generally and to a limited extent, adverse reactions to medication are positively intercorrelated throughout the three modalities studied: side-effects, resistance, and dosage deviation.

EXTREME DOSAGE DEVIATORS

In two senses, the adverse reactors in this study are a selected group. They do not include 1) patients who terminated treatment prematurely, and 2) patients who completed the study but were thereafter determined, on the basis of the physicians' records, to have deviated so excessively from the prescribed dosage that they had to be eliminated from the sample before the data were analyzed.[7] Many of the 46 patients in the first category disappeared from treatment without con-

[7] Any patient was so eliminated if he had been without medication for 1) a total of more than 21 days scattered through the eight-week treatment period, or 2) 14 consecutive days, or 3) two or more periods, each of not less than four consecutive days, totaling 14 days.

sulting either their prescribing physicians or their psychotherapists. Hence it was impossible to obtain adequate information on their adverse reactions to medication, particularly during the crucial period between their latest visit and their decision to leave treatment.

Raskin (6) made a study of 37 patients who were excessive dosage deviators and who were therefore eliminated from the sample even though they completed the full eight weeks of treatment. (The term *resistors* in Raskin's title (6) is synonymous with our term *deviators*.) Some of these patients frankly refused medication after starting treatment and were formally removed from it, but remained in psychotherapy, the primary treatment, and underwent the usual research evaluations. Raskin compared the 37 extreme deviators on a large number of assessment variables with the 142 medication patients in our main sample. The outstanding difference was the greater degree of overt hostility and aggression in the extreme deviators. Several of the specific comparisons pertaining to the treatment experience of these patients are relevant to the present study. At the end of the first psychotherapy interview, immediately after being told they were to receive medication, the 37 extreme deviators were rated by their therapists as less positive in their attitude to medication (eager vs. reluctant) than the patients in the main sample. In another rating made at the same time, the extreme deviators' therapists were less optimistic, as compared with the other 142 patients' therapists, regarding the extent to which a tranquilizer could be expected to facilitate psychotherapy. After finishing their eight weeks of treatment, the extreme deviators scored higher than did the main sample on the sum of two highly correlated therapist ratings of overt and covert resistance to psychotherapy. Each of the three differences noted here was significant by t test at the .05 level. One other comparison by Raskin, which proved not to be significant, is also of interest: at the end of the first week of treatment, the mean number of side-effects reported by physicians for the extreme deviators was essentially no different, by t test, from the one-week mean for the main sample.

DISCUSSION

We have investigated a few of the more obvious antecedents of, and relationships between, three modes of adverse reaction to psychopharmacologic drugs: side-effects, attitudes of resistance, and dosage deviations. The main line of evidence shows traces of psychologically plausible consistency in the patterning of these variables. On the other hand, direct differential pharmacologic effects of the drugs on adverse reactions were not demonstrable; essentially it is as though all patients had been taking the same medicine. (The same was true, with few exceptions, for therapeutic effects in the major study (4) from which these data were drawn.)

It cannot be claimed, however, that the pharmacologic effects on adverse reactions were therefore negligible. It is much more likely that such effects were mediated by interaction with a number of other variables not examined in this study. One such possible mediator is the patient's "biochemical individuality" (8). Another is the treatment milieu, including but not limited to the doctor-patient relationship. A third is the patient's personality, which is already implicated in other studies of positive response to placebo, but which is thus far comparatively neglected in the study of negative placebo response and of drug effects positive or negative.

One question which arises in the study of adverse reactions is, What is their relation to treatment outcome? For example, are side-effects associated with improvement, with failure to improve, or with neither? To what extent does a resistive attitude obstruct benefit from active medication and from placebo? Again, these questions have not

been dealt with here, but would have to be considered in a comprehensive treatment of adverse reactions.

Finally, it is important that the findings of this study not be overgeneralized. For example, the dispensing of medication by a physician who was not the psychotherapist may have engendered a complaining attitude which endowed the placebo patient with about as many "side-effects" as the active drug patient showed. Undoubtedly the low dosage levels of the active drugs also militated against a drug-vs.-placebo difference in side-effects. A third factor which may have blurred this distinction is that our tabulations did not take into account the persistence or non-recurrence of side-effects beyond the visit at which they were initially noted. Conceivably, if side-effects had been weighted to reflect their duration, placebo patients might have scored lower than active-drug patients.

More generally, our findings obviously do not apply invariantly to patients who are hospitalized, or to outpatients who terminate treatment during the first eight weeks. On the other hand, such limitations as we have noted need not obscure the implications of the study for further inquiry into the basic mechanisms of response to drugs.

SUMMARY

The subjects of this eight-week study were 142 male psychotherapy outpatients in Veterans Administration mental hygiene clinics, each of whom received, on a double-blind basis, one of four medications: chlorpromazine, meprobamate, phenobarbital and placebo.

The total number of side-effects per patient, ranging from zero to 14, with a median of 1.15, was not differentially related to the kind of medication received. The same was true for the frequency of those single side-effects common enough to be tested statistically.

The eight most common side-effects, nearly all of which were significantly correlated one with another in the total patient sample, were: 1) drowsiness, lethargy; 2) tension; 3) restlessness, excitement; 4) weakness, fatigue; 5) vivid or frequent dreams; 6) nausea, GI discomfort; 7) dryness of mouth or throat; and 8) headache or "hang-over" effect.

There was no demonstrable consistent relationship between specific presenting symptoms and the occurrence of matching side-effects.

Emotional resistance to medication was typically associated with subjective or evaluative notions of its actual or anticipated effect, communicated to the physician by the patient.

Deviations from the prescribed dosage, *e.g.*, taking less than prescribed, were usually considered by the physician to have been involuntary. Typically, they were related to irregularities or changes in the patients' daily routines. A minority of deviations were judged to have been "motivated" by emotional resistance.

Pre-treatment attitude to medication (degree of eagerness) was not related to resistance or to dosage deviation, but was related nonlinearly to number of side-effects.

Resistance and dosage deviations were positively correlated at a high level of significance, and were both, like side-effects, unrelated to the kind of medication received. The relation between side-effects and dosage deviation was positive and significant; the relation of side-effects to resistance, also positive, approached significance.

REFERENCES

1. Beecher, H. K. The powerful placebo. J.A.M.A., **159**: 1602–1606, 1955.
2. Cohen, I. M. Undesirable effects and clinical toxicity of chlorpromazine. In Gordon, H. L., ed. *The New Chemotherapy in Mental Illness*, pp. 50–93. Philosophical Library, New York, 1958.
3. Friedman, H. T. and Marmelzat, W. L. Adverse reactions to meprobamate. In Gordon, H. L., ed. *The New Chemotherapy in Mental Illness*, pp. 684–687. Philosophical Library, New York, 1958.
4. Lorr, M., McNair, D. M., Weinstein, G. J.,

MICHAUX, W. W. AND RASKIN, A. Meprobamate and chlorpromazine in psychotherapy: Some effects on anxiety and hostility of outpatients. A.M.A. Arch. Gen. Psychiat., **4**: 381–389, 1961.

5. MICHAUX, W. W. AND CIBELLI, L. A. Side effects. In *Transactions of the Fifth Research Conference on Chemotherapy in Psychiatry*, pp. 48–50. Veterans Administration, Department of Medicine and Surgery, Washington, D. C., 1960.
6. RASKIN, A. A comparison of acceptors and resistors of drug treatment as an adjunct to psychotherapy. J. Consult. Psychol., **25**: 366, 1961.
7. SARWER-FONER, G. J. The transference and nonspecific drug effects in the use of the tranquilizing drugs, and their influence on affect. Psychiat. Res. Rep. Amer. Psychiat. Ass., **8**: 153–167, 1958.
8. WILLIAMS, R. J. Chemical anthropology—an open door. Amer. Scientist, **46**: 1–23, 1958.
9. WOLF, S. AND PINSKY, R. H. Effects of placebo administration and occurrence of toxic reactions. J.A.M.A., **155**: 339–341, 1954.

MEMORY FUNCTION OF PATIENTS TAKING ETRYPTAMINE ACETATE

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Impairment of memory function reportedly occurs during treatment with antidepressive pharmaceutic agents to a lesser extent than after treatment with electroconvulsive therapy (4). Etryptamine acetate (Monase) is such a new antidepressant, and patients taking it occasionally complain of memory loss. Animal experimentation also indicates that high dosages of etryptamine acetate (100 mg./Kg. in rats) may result in various degrees of disorientation (2). We made tests with patients on etryptamine acetate using the Wechsler Memory Scale (Forms 1 and 2) to demonstrate any effect quantitatively.

METHODS

Twenty women, aged 22 to 68 years (46.7 ±3.1 years[2]), participated in the study. They were acutely ill psychiatric patients requiring antidepressant and/or stimulatory chemotherapy. Their etryptamine acetate dosage was adjusted until a "therapeutic" level was reached. This dosage (40.5 ± 5.8 mg./day, by mouth) was maintained for at least one week.

The pre-test therapy was given so that patients could adjust to the medication, physiologically and psychologically, but, to minimize its effect on the first part of the test, was withheld on the day before testing. This permitted nearly maximal excretion. The drug-free period before testing was 40 hours, and it has been shown that from 64 to 69 per cent of C^{14}-labeled etryptamine acetate is excreted in the urine within 24 hours, with a maximum recovery over a two-week period of about 72 per cent of the administered dose (1).

The study was designed to compare memory function before and after a single dosage of etryptamine acetate. The patients were alternated between two groups: Group 1 was tested first before and after the drug; a week later, before and after placebo. In Group 2 this sequence was reversed. Each group was divided further into two subgroups, designated as Condition A and Condition B. Condition A indicates that the MQ Form 1 was administered before drug or placebo, followed by MQ Form 2. In Condition B, MQ Form 2 was followed by MQ Form 1. The testing sequence is outlined in Table 1. The pre-drug or pre-placebo MQ tests were administered at 9:30 a.m. and were followed immediately by drug or placebo. In Groups 1 and 2 (again divided evenly under Conditions A and B), six patients received approximately their total daily dosage of etryptamine acetate; the remaining four received three to four times their usual daily dosage. The higher drug dosage was used in an attempt to force an impairment of memory function if this drug really could cause any. The data for those receiving approximately their daily dosage and for those receiving a higher dosage were reviewed separately. Since no significant differences were observed, the data are here combined. Group 1 patients received 61.5 ± 16.6 mg., and Group 2 patients, 66.0 ± 13.5 mg. etryptamine acetate on the day of memory testing under drug. On the day memory function was tested under placebo, the same number of placebo tablets was given to each patient.

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[2] Mean ± Standard Error.

TABLE 1

Testing Sequence

	Group 1		Group 2	
	Condition A N = 5	Condition B N = 5	Condition A N = 5	Condition B N = 5
Etryptamine in "therapeutic" dosage for at least six days (40 hours medication-free)				
First testing day	MQ Form 1 Drug	MQ Form 2 Drug	MQ Form 1 Placebo	MQ Form 2 Placebo
	1-hour interval			
	MQ Form 2	MQ Form 1	MQ Form 2	MQ Form 1
Etryptamine in same "therapeutic" dosage for six days (40 hours medication-free)				
Second testing day	MQ Form 1 Placebo	MQ Form 2 Placebo	MQ Form 1 Drug	MQ Form 2 Drug
	1-hour interval			
	MQ Form 2	MQ Form 1	MQ Form 2	MQ Form 1

TABLE 2

Results of Memory Quotient Tests

	N	Drug Testing		Placebo Testing		Average Post-test Minus Pre-test Scores		
		Pre-drug Mn ± SE	Post-drug Mn ± SE	Pre-placebo Mn ± SE	Post-placebo Mn ± SE	Drug Testing	Placebo Testing	Both Testings Combined
		(1st test day)		(2nd test day)				
Group 1								
Condition A*	5	114.0±15.1	101.8±11.2	120.0±17.8	111.8±12.1	−12.2	−8.2	−10.2
Condition B†	5	87.4± 8.4	96.8±11.6	87.2±11.4	90.2±14.3	+9.4	+1.4	+5.4
Both	10	100.7± 9.3	99.3± 7.6	103.6±11.4	101.0± 9.5	−1.4	−3.4	−2.4
		(2nd test day)		(1st test day)				
Group 2								
Condition A*	5	89.8±11.4	84.4±10.2	89.2± 3.1	87.4± 6.4	−5.4	−1.8	−3.6
Condition B†	5	109.0± 9.9	109.8±11.0	100.2± 7.9	101.2± 6.8	+0.8	+1.0	+0.9
Both	10	99.4± 7.8	97.1± 8.2	94.7± 4.4	94.3± 5.0	−2.3	−0.4	−1.4
Group 1 and 2 Combined		100.1± 5.9	98.2± 5.5	99.2± 6.0	97.7± 5.3	−1.9	−1.9	−1.9

* Condition A (MQ Form 1, followed by MQ Form 2).
† Condition B (MQ Form 2, followed by MQ Form 1).

RESULTS

The results are summarized in Table 2. Pre-test minus post-test scores, averaged over all of the experimental groups (last line of the table), were identical for drug and placebo, indicating no drug effect. The first-day pre-test scores did not differ significantly between groups having the same

TABLE 3

Analysis of Variance of Post-test minus Pre-test scores

Effect	D.F.	S.S.	M.S.	F*	p
A: Drug vs. Placebo	1	0.03	0.03	<1	n.s.
B: Drug-Placebo Order	1	11.03	11.03	<1	n.s.
C: MQ Form Order	1	1010.03	1010.03	25.51	<0.001
D: Between *Ss* (within groups)	16	3364.80	210.30	5.31	0.001
A × B	1	38.03	38.03	—	—
A × C	1	148.00	148.00	3.74	n.s.
B × C	1	308.03	308.03	7.78	0.05
A × B × C	1	46.45	46.45	—	—
A × D	16	628.00	39.25	—	—
Total	39	5554.40	—	—	—

* A × B, A × D, and A × B × C were pooled for error.

form. There were no significant differences between pre-test scores and scores on the two testing days. An analysis of variance of the post-test minus pre-test scores is given in Table 3. The only significant main effect other than patient differences is "form order." MQ Form 2 was more difficult.

A few patients complained of dizziness, but no serious side-effects were observed or reported.

Although these patients, like those reported in the literature (4), complained of difficulty in thinking, concentrating and remembering during the test situation, etryptamine acetate therapy was not accompanied by objectively measurable impairment of memory function. Perhaps preoccupation with the test situation or with mild side-effects alters mental responsiveness sufficiently to cause the subjective complaints without affecting the objective measurement of memory function.

SUMMARY AND CONCLUSION

Because antidepressant therapy has reportedly been associated with memory impairment, we tested 20 patients with the Wechsler Memory Scale (Forms 1 and 2) to determine whether etryptamine acetate (Monase) would affect the scores. Patients took two MQ tests on each of two test days under similar conditions, except that one-half of the patients received etryptamine acetate an hour before the second test, and one-half received a similar number of placebo tablets identical in appearance. Six patients in each group received the number of tablets about equal to their usual daily quota of etryptamine acetate tablets, and four received three to four times more than that number. The group receiving the drug on the first test day received the placebo on the second, and vice versa. Scores were similar after etryptamine acetate or placebo whether the usual daily dose was taken, or up to four times that dose.

We conclude that etryptamine acetate has no effect on memory function under the conditions of the experiment.

REFERENCES

1. Eberts, F. S. Metabolic studies with 3-(2-aminobutyl-1-1-C_{14}) indole acetate [Monase-C_{14}]. Distribution and excretion in rat, dog, and man. J. Neuropsychiat. (Suppl. 1), **2**: 146–150, 1961.
2. Gray, J. E. Toxicologic effects of etryptamine in experimental animals. J. Neuropsychiat. (Suppl. 1), **2**: 163–165, 1961.
3. Greig, M. E., Seay, P. H. and Freyburger, W. A. The pharmacology of etryptamine. J. Neuropsychiat. (Suppl. 1), **2**: 131–135, 1961.
4. Hankoff, L. D. and Heller, B. Memory changes with MAO inhibitor therapy. Amer. J. Psychiat., **117**: 151–153, 1960.

EXPRESSED FEAR OF HYPNOSIS AND VOLUNTEERING BEHAVIOR

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Experimental studies in medical psychology using human volunteer subjects are often confounded by sampling bias. In the last few years, some of the personality characteristics of volunteer subjects and other factors which influence volunteering behavior have been investigated (2–4). Studies involving hypnosis pose a special problem because of the diverse and often odd notions held by laymen about it. The ideas and attitudes of potential volunteers are derived from many sources: common folklore about hypnosis; previous experience with hypnosis (usually in a parlor-game atmosphere) as hypnotist, subject or spectator; and, of course, the late show on television, which portrays the hypnotist as a bearded Svengali who controls the lives of others by strange machinations.

Apprehension about hypnosis, perhaps arising from such misconceptions, might well influence a subject's disposition to volunteer for a hypnotic experiment. In this study the relationship of *Ss*' expressed fear of hypnosis to their general anxiety level, information about hypnosis, and willingness to participate in a hypnotic experiment was investigated.

PROCEDURE

The following psychologic tests were given to a sophomore class of 76 student nurses in a group situation:

1) The IPAT Anxiety Scale, a self-administered questionnaire of 40 items to assess a general factor of anxiety. Cattell (1) has reported extensive normative data on the instrument and information about its reliability and validity.

2) The Hypnosis Information Inventory, a 25-item questionnaire to determine the correctness of *S*'s information about hypnosis. (For example, the statement, "It is difficult to hypnotize anyone without having him stare at a shiny object," must be checked as *true*, *probably true*, *probably false* or *false*, and scored as 1, 2, 3 or 4.)

3) A sentence to be completed: "I think I would be ———— to be hypnotized," with "afraid," "somewhat afraid" or "unafraid."

Two weeks after this group testing the class was again assembled and asked to volunteer for a psychologic experiment involving hypnosis. They were told, in very general terms, the time involved, the remuneration, and the nature of the experiment. Details of the experimental procedure and the purpose of the study were not disclosed. Each student indicated privately on a slip of paper whether she wanted to participate. Later, each nurse who had indicated a desire to volunteer was given an appointment for the experiment proper. Those who had indicated a desire to participate in the group situation and then followed through when again notified were classified as "volunteers." Those who did not indicate a desire to participate, or who failed to appear for the scheduled appointment for the experiment proper, were classified as "non-volunteers."

There were 38 volunteers and 38 non-volunteers. Of the latter, 16 had indicated a desire to participate in the experiment but failed to follow through.

RESULTS

Table 1 shows the results of a chi-square test of association between expressed fear of hypnosis and volunteering behavior. Since only six *Ss* had indicated they were "afraid" to be hypnotized, they were grouped with

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TABLE 1

Classification of Subjects According to Expressed Fear of Hypnosis and 1) Volunteering Behavior, 2) General Anxiety Level and 3) Information About Hypnosis

Expressed Fear of Hypnosis	Volunteering Behavior		IPAT Anxiety Scale		Hypnosis Information Inventory*	
	Volunteer	Non-volunteer	High Anxiety (above median)	Low Anxiety (below median)	Misinformed (above median)	Informed (below median)
Unafraid	23	7	16	14	15	15
Somewhat Afraid and Afraid	15	31	24	22	23	21
	$X^2 = 14.1$	$p < 0.001$	$X^2 = 0.01$, n.s.		$X^2 = 0.04$, n.s.	

* Hypnosis Information Inventory N is 74 because two *Ss* failed to complete this test.

those who had answered "somewhat afraid" to the same question, making a total of 46 in this category. Note that the association between expressed fear of hypnosis and failure to volunteer is very strong ($p < .001$).

Table 1 also shows the results of a chi-square test of association between expressed fear of hypnosis and 1) the IPAT Anxiety Scale total score and 2) the Hypnosis Information Inventory score. No association is seen in either case.[2]

DISCUSSION

Overt apprehension at the prospect of being hypnotized appears to be an important variable in determining volunteering for psychologic experiments involving hypnosis. Thus, fear of hypnosis is one origin for the sampling bias introduced into such experiments. The lack of association between expressed fear of hypnosis and the general anxiety level of the subject, as revealed by the IPAT Anxiety Scale, suggests that the hypnosis anxiety has some specificity in these subjects. Further, the lack of association between expressed fear of hypnosis and the accuracy of *S*'s information about hypnosis, as revealed by the Hypnosis Information Inventory, suggests that the hypnosis anxiety is not simply the product of gross misconceptions about hypnotic phenomena. The anxiety generated in some *Ss* at the prospect of being hypnotized is probably related to the psychologic meaning, both conscious and unconscious, of submitting to hypnotic trance. Further studies are in progress to identify personality and other factors that influence volunteering for psychologic experiments involving hypnosis.

[2] Again with the use of chi-square, no significant association was found between volunteering behavior and 1) the IPAT score or 2) the Hypnotic Information Inventory score.

REFERENCES

1. Cattell, R. B. *Handbook for the IPAT Anxiety Scale*. Institute for Personality and Ability Testing, Champaign, Illinois, 1957.
2. Howe, E. S. Quantitative motivational differences between volunteers and nonvolunteers for a psychological experiment. *J. Appl. Psychol.*, **44**: 115–120, 1960.
3. Martin, R. M. and Marcuse, F. L. Characteristics of volunteers and nonvolunteers in psychological experimentation. *J. Consult. Psychol.*, **22**: 475–479, 1958.
4. Riggs, M. M. and Kaess, W. Personality differences between volunteers and nonvolunteers. *J. Psychol.*, **40**: 229–245, 1955.

EVIDENCE FOR HYPNOTICALLY INDUCED AMNESIA AS AN ANALOG OF REPRESSION

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Hypnotically induced amnesia is sometimes regarded as an analog of repression. Gordon (2) states that "the hypnotic instructions to forget... produce behavior analogous to repression...," and Bobbitt (1) suggests that the hypnotic method can be employed to "create the effects of repression."

The analogy between hypnotic amnesia and repression appears to be logical, but it has been subjected to very little experimental testing, nor has the potential connection been utilized for the objective study of repression. Only one (3) of the studies in Zeller's review (7) of experimental investigations of repression utilized the hypnotic method. One reason for this neglect may be that most practitioners (who make up the majority of professional persons interested in hypnosis) are more concerned with hypermnesia because of its greater therapeutic and diagnostic value.

An experimental demonstration of the analogy requires more than simply a forgetting followed by a recalling of events or verbal material, each under stipulated conditions. The dynamic implications require that hypnotic amnesia affect subsequent behavior in the same way as might be predicted if memory for events was actually repressed. The present paper is a report of what appears to be such a demonstration.

SUBJECTS AND PROCEDURE

The subjects were nine sophomore student nurses, all 19 years old, and all volunteers paid for their participation in each experiment. Each was able to attain easily a hypnotic state of at least moderate depth by usual standards, including positive response to the suggestion of amnesia for events.

A diffuse anxiety state was induced in the *Ss* under hypnosis in two experiments about six months apart. The technique is described elsewhere (4). In each instance *S* was made amnesic for the anxiety state after the experiment was concluded.

It is of course possible to fake amnesia, and there is no certain way to uncover faking. The impression that our *Ss* were not faking is based on three considerations: First, amnesia was manifested by all *Ss* in a pre-experimental training period, when none was likely to be aware that it would be induced after the experiment. Second, none was able to recall an anxiety experience of the first experiment when questioned later. Third, *Ss* volunteered for the second experiment even though they were told it would be similar to the first.

During the course of each experiment, a series of measurements were obtained on the *Ss*, once before the induction of anxiety and again during anxiety. The measures were Taylor's Manifest Anxiety Scale (6), presumably a fairly stable index of anxiety level; Zuckerman's Affect Adjective Check List (8), a measure of the immediate feeling of anxiety; clinical ratings of manifest anxiety on a six-point scale by two judges observing through a one-way vision screen; and the level of hydrocortisone in the plasma, which has been found to increase with anxiety in normal *Ss* (5). In Experiment 1 the judges were a psychiatrist and a psychologist; in Experiment 2, two psychiatrists.

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TABLE 1

Mean Scores on the Affect Adjective Check List, the Manifest Anxiety Scale, and Plasma Hydrocortisone Level

	AACL Experiment		MAS Experiment		Hydrocortisone Experiment*	
	1	2	1	2	1	2
Pre-anxiety	4.22	4.89	12.22	14.89	16.61	24.44
During anxiety	17.67	18.44	19.78	24.56	19.80	21.07
Mean	10.95	11.67	16.00	19.73	18.21	22.76

* $N = 7$.

TABLE 2

Mean Clinical Anxiety Ratings

Experiment 1 Judges			Experiment 2 Judges		
A	B	Mean	C	D	Mean
1.56	1.56	1.56	2.72	2.61	2.67

$r = .90$ $r = .89$

$r_{1-2} = .86$

RESULTS

All mean scores are shown in Tables 1 and 2. Plasma levels of hydrocortisone were not obtained for one *S* in Experiment 1, and data for another subject were so atypical as to suggest measurement error.[5] Results for the latter were discarded, and hence N = 7 for the hydrocortisone data.

In almost all cases in both experiments the pre-anxiety clinical ratings were zero and hence are not presented. The correlations between ratings for the judges were .90 for Experiment 1, .89 for Experiment 2, and .86 between means for the experiments.

All mean scores show the same inter-experiment trend. *Ss* score higher on the second experiment on all measures; all Experiment 2 pre-anxiety measures are higher than the comparable Experiment 1 mean, and all Experiment 2 during-anxiety measures are higher than in Experiment 1.

[5] In Experiment 2 this *S* manifested a decrease in hydrocortisone of nearly 90 per cent of her Experiment 1 level. Most *Ss* show an increase, and any decreases do not exceed 10 per cent of the level of the previous experiment.

TABLE 3

Variance Analysis Summaries of AACL and MAS Data

Source	*df*	AACL Mean Square	AACL *F*	MAS Mean Square	MAS *F*
Subjects	8	5.74	—	341.07	—
Experiments	1	4.70	0.76	124.70	6.41*
States	1	1640.25	112.12†	667.37	5.57*
Exp × St	1	0.02	0.00	10.02	2.49
Exp × Subj	8	6.18	—	19.44	—
St × Subj	8	14.63	—	19.74	—
St × Subj × Exp	8	7.54	—	4.03	—

* Significant at the .05 level.
† Significant at the .001 level.

TABLE 4

Variance Analysis Summary of Clinical Anxiety Ratings

Source	*df*	Mean Square	*F*
Subjects	8	4.14	—
Experiments	1	11.12	31.77*
Judges (1)	1	0.00	0.00
Judges (2)	1	0.06	0.29
Exp × Subj	8	0.35	—
Subj × J (1)	8	0.09	—
Subj × J (2)	8	0.21	—

* Significant at the .001 level.

TABLE 5

Variance Analysis Summary of Hydrocortisone Data

Source	*df*	Mean Square	*F*
Subjects	6	80.46	—
Experiments	1	144.92	3.84*
States	1	0.06	0.00
Exp × States	1	75.24	1.82
Exp × Subj	7	37.73	—
St × Subj	7	21.65	—
St × Subj × Exp	7	41.31	—

* $p = .10$.

Differences among means were evaluated by analyses of variance. Summaries are found in Tables 3, 4 and 5. The appropriate error term for the F ratios of differences between means for experiments is the mean square for the Experiment × Subject interaction. The error for evaluating states (*i.e.*, before and during) is the States × Subject

interaction, and the triple interaction (States × Subjects × Experiments) is the appropriate error for evaluating the Experiment × States interaction. No other interaction can be evaluated statistically in this design.

Mean differences between experiments are significant for the MAS (16.00 and 19.73) and the clinical ratings (1.56 and 2.67), and reach the .10 level for plasma hydrocortisone (18.21 and 22.76). The difference for the AACL (10.95 and 11.67) is in the same direction as the others. No Experiment × State interaction is significant, and differences between pairs of judges are also not significant.

DISCUSSION

The results which bear on the analogy between hypnotically induced amnesia and repression are the inter-experiment differences. On the assumption that the *S*s were truly amnesic for the anxiety experience of Experiment 1, how are the general increases in anxiety indices in Experiment 2 most parsimoniously explained? Evidently we must postulate some kind of carry-over effect from Experiment 1. Since the carry-over was not on the conscious level, it was most probably on an unconscious level. The analogy with repression of a traumatic incident and its effect on later behavior need not be belabored.

The decrease in hydrocortisone level from the pre-anxiety level to the during-anxiety state in Experiment 2 requires a word of explanation. This is a phenomenon which seems to be peculiar to this measure. The normal *S* with normal hydrocortisone level will manifest an increase with hypnotically induced anxiety. But if, for some reason, *S enters* the experiment with an elevated level, there is a tendency for the level to *decrease* with induced anxiety. The explanation of this phenomenon probably has to do with relationships among hydrocortisone level, its metabolism and personality characteristics, but it is not pertinent to our argument. The phenomenon inflates the interaction between states and experiments, and thus probably neutralizes a potential inter-experiment increase in mean scores. However, the difference between *pre-anxiety* means for hydrocortisone for the two experiments (16.61 and 24.44) is significant ($t = 3.14$, $p < .01$), a fact which supports the repression analogy hypothesis.

We do not maintain that the results reported here are convincing beyond any reasonable doubt. The data are fairly, but not completely, consistent with the analogy hypothesis. Furthermore, the bearing of these data on the analogy hypothesis depends directly upon acceptance of the phenomenon of hypnotically induced amnesia among our subjects. Our impression is that such amnesia actually occurred, but we cannot offer any independent evidence for it. A more convincing demonstration would be the offer of an inducement to *S*s to recall details of the experiment. The procedure is suggested for further work in this area.

SUMMARY

A group of normal students were subjected to hypnotically induced anxiety on two separate occasions, with hypnotically induced amnesia for the first experience. Scores on four measures of anxiety tended to be higher on the second experiment than on the first. These findings are cited as evidence that hypnotically induced amnesia is an experimental analog of repression.

REFERENCES

1. Bobbitt, R. A. The repression hypothesis studied in a situation of hypnotically induced conflict. J. Abnorm. Soc. Psychol., **56**: 204–212, 1958.
2. Gordon, J. E. Leading and following psychotherapeutic techniques with hypnotically induced repression and hostility. J. Abnorm. Soc. Psychol., **54**: 405–410, 1957.
3. Huston, P. E., Shakow, D. and Erickson, M. H. A study of hypnotically induced complexes by means of the Luria technique. J. Gen. Psychol., **11**: 65–97, 1934.
4. Levitt, E. E., den Breeijen, A. and Persky, H.

he induction of clinical anxiety by means of standardized hypnotic technique. Amer. J. lin. Hypnosis, **2**: 206–214, 1960.

5. P SKY, H., GROSZ, H. J., NORTON, J. A. AND ICMURTRY, M. Effect of hypnotically induced nxiety on hydrocortisone level of normal ubjects. J. Clin. Endocr., **19**: 700–710, 1959.

6. T YLOR, J. A. A personality scale of manifest anxiety. J. Abnorm. Soc. Psychol., **48**: 285–290, 1953.

7. ZELLER, A. F. An experimental analogue of repression. I. Historical summary. Psychol. Bull., **47**: 39–51, 1950.

8. ZUCKERMAN, M. Development of an affect adjective check list for the study of anxiety. J. Consult. Psychol., **24**: 457–462, 1960.

PERCEPTION OF A THREE-DIMENSIONAL SPATIAL INCONGRUITY BY BRAIN-DAMAGED AND OTHER ADULTS[1]

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Differences in space perception have been linked to many factors, among them race (13), visual exposure to stimulating and complex early environments (7) and training (5). One aspect of space perception—the ability of the human organism to perceive three-dimensional space, even though it is only implied in two-dimensional pictorial representation—appears to parallel general development. Thus, Updegraff (16) reports that four-year-old children, even those sophisticated in spatial orientation, cannot interpret the representation of space in terms of such attributes as perspective, relative size and shadows. Behar and Bevan (3) used a perceptual incongruity to investigate development of this ability in children ranging from four through seven years of age. They employed a photograph of a three-dimensional building, taken so that it appeared to be two-dimensional (Figure 1), and a structured inquiry which could be scored reliably. The obtained score depended upon the number of cues required for correct identification of the incongruity and the clarity with which it was expressed. They found their four-year-olds scored uniformly low, with perception of the incongruity increasing monotonically with chronologic and mental age, so that more than half of their six- to seven-year group required three cues or less from the examiner. In a second study, in which white college students participated (4), all were able to identify the incongruity without difficulty. In a sense we have a developmental curve and, although we have relatively few points on this curve from which to generalize, the ability to perceive even implied three-dimensionality seems to appear developmentally relatively late in childhood, and to be present almost uniformly in acculturated young white adults.

Impairment has also been noted in the ability of brain-damaged adults to perceive three-dimensional space. Reviewing studies of visual disturbances following cerebral lesions in World War I casualties, Klüver (11) cited work by Gelb of a condition he called "dysmorphopsia." Patients saw near objects normally. When distance was increased, they reported characteristic changes in object form. Klüver also reviewed Poppelreuter's work with forms presented tachistoscopically at varying visual angles. With visual angles of less than 50°, normal subjects had no difficulty in recognition of forms, whereas the brain-injured obviously did (11, p. 336). Neither of these experimenters nor Klüver mentions impairment in patients' ability to deal with three-dimensional space as a factor, although it would seem a reasonable inference. Goldstein (9) describes a space visualization task in which a brain-injured adult is asked to throw a ball into open boxes situated 3′, 9′ and 15′ apart. He is able to perform the task but cannot report how far away each box is, or which box is nearest and which farthest. Goldstein used this to support his thesis that the abstract attitude is impaired. Without in any way detracting from Goldstein's view, we feel this could also be looked upon as reflecting impairment in three-dimensional spatial visualization.

In studies of visual constructional apraxia,

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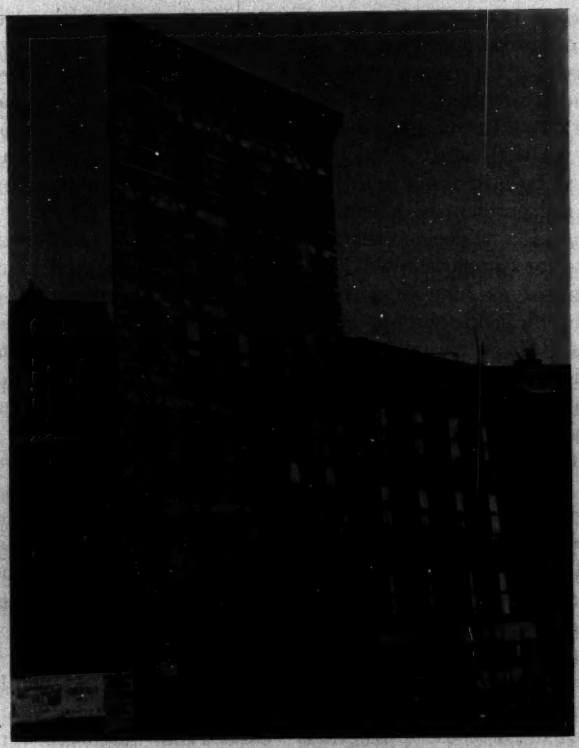

FIG. 1. Incongruities Picture

Hecaen and co-workers (as reviewed by Bay (1)) found that patients with right hemisphere damage were relatively unable to represent perspective in drawings of three-dimensional objects. Critchley (cited by Ettlinger *et al.* (6)), on the other hand, concluded that generalized impairment in this and other processes can be found in bilateral cases as well as in those with lesions of either hemisphere. These authors and others (12, 14) continue the study of hemisphere role in visual-spatial perception, but our concern is primarily with impairment implied in perceptual functions themselves. It seems reasonable to conclude from these studies that ability to deal with three-dimensional space is impaired in the brain-damaged person.

Support for this conclusion comes from yet another source, the correlational work of Gordon and Tikofsky (10). Their factor analysis of the performance of brain-damaged adults on the Gottschaldt Embedded Figures Test revealed the presence of a three-dimensional space factor.

The performance of brain-damaged individuals and of young children may reveal many similarities in dealing with pictorially represented three-dimensional space. The present study was designed primarily to investigate whether brain-damaged adults suffer impairment in implied three-dimensionality as compared with other adults. If differential impairment was demonstrated, could it be related to earlier developmental work with children on the same task? While direct comparisons between adults and children are admittedly difficult (particularly as to such factors as complexity-simplicity

of a task, changes in instructions required, and the inherent interest it may have for the populations tested), the perceptual incongruities task and structured inquiry developed by Behar and Bevan appeared to be well suited to use with adults.

The study was to be carried out in a clinical setting, and the problem of a suitable control group arose. It was decided that one group appropriate for comparison would be individuals diagnosed as functional disorders other than chronic schizophrenics or depressive reactions. Because chronic, or process, schizophrenics are different in many ways from other psychiatric disorders (2, 15), it was deemed wiser to exclude them. Also, because perceptual-motor functioning is impaired in a fairly large percentage of the clinically depressed, it was felt justifiable to make a separate group of these patients. Our predictions were that the so-called Functional "Other" group would require fewer cues to identify verbally the perceptual incongruity than would the Depressed, who in turn would require fewer cues for verbal identification than would the Brain-Damaged. If these predictions were supported, other psychometric measures were available for many of the patients which might contribute to an understanding of the processes underlying the difference.

METHOD

Subjects (*Ss*)

One hundred two male veterans in a general medical hospital, routinely referred for psychologic evaluation, comprised the *S* pool. The perceptual incongruities test was a research addition to whatever procedures the psychologist used for clinical purposes. Incongruities test results were filed in a research folder and were not used in assessment or diagnosis of these patients. Seventy-four *Ss* met the diagnostic criterion for inclusion, which was the final (discharge) medical diagnosis. *Ss* were assigned to groups (by PGD) without knowledge of incongruity test scores.

Functional "Other" group (N = 32): These were psychiatric patients. Diagnoses were based on results of psychiatric examinations, laboratory findings, and consultative reports (including psychologic and social services). All final diagnoses were reviewed by a board-certified psychiatrist. The disorders covered were: 13 acute schizophrenias, two obsessive-compulsive reactions, ten anxiety reactions, two psychophysiologic reactions, two conversion reactions and three character disorders. Excluded were chronic schizophrenias and affective reactions.

Depressive group (N = 21): These were also psychiatric patients. Final diagnoses were arrived at and reviewed in the same way as for the Functional "Other" group. Only depressive reactions and manic-depressive psychoses, depressed type, were included.

Brain-Damaged group (N = 21): These were referred from several sources, but neurologic consultation was carried out with each. Final diagnoses were based upon extensive examinations, laboratory findings and consultative reports. Four final summaries contained postmortem confirmation. Also, records of prior hospitalization were available for a number of *Ss*. Seventeen *Ss* had at least one EEG study during this hospitalization, three had pneumoencephalograms, and three had arteriographic procedures. All had been evaluated with psychologic techniques.

The group included seven *Ss* with chronic brain syndromes, two of which were associated with trauma; four with acute brain syndromes, all associated with alcohol; four with encephalopathies, each due to trauma; two with cerebrovascular accidents; two with generalized arteriosclerosis, and two with brain tumor (confirmed post mortem).

The groups differed significantly in age. The *F*-ratio, with two and 71 degrees of

freedom, respectively, was 8.16, which yields a p value beyond the .001 level of confidence. The Functional "Other" group, with a mean age of 34, was significantly younger than either the Brain-Damaged or the Depressed, each with a mean age of 43. There was, however, no statistically reliable relationship between age and incongruities score for the entire sample ($r = .09$) or for the groups separately (the rs ranging from $-.09$ to $+.12$).

STIMULUS MATERIAL

Sensitivity to perceptual incongruity was measured by rating the patient's responses to a photograph, 3½ x 4½", of a three-sided building of the "flat-iron" type, mounted on a stiff cardboard mat (Figure 1). The photograph had been taken so that only the front of the building was visible, yielding the impression of a two-dimensional façade. It was not retouched.

PROCEDURE

The incongruities test was administered at the time of initial referral, almost always within two weeks of admission. The procedure was exactly the same as that employed by Behar and Bevan in their developmental study: the patient was shown the photograph and was asked, "Tell me what you see in the picture." If awareness of the perceptual incongruity was not verbalized, he was asked, "Is there anything odd or unusual about the picture?" If this did not elicit recognition, the experimenter (E) ran his finger vertically over the incongruous building and asked, "Is there anything odd or unusual about this building?" If there was still no response that could be scored, the patient was asked a series of progressively leading questions until finally very direct questions relating to the incongruity itself were asked. If there was any impression of recognition (*i.e*, a response that could be scored), E attempted to elicit an explanation, a reason for the incongruity. The patient was asked, "Why does the building look like it does?" and "How did it get that way?" All verbal responses were recorded verbatim.

MEASUREMENT OF SENSITIVITY TO INCONGRUITY

Protocols were scored (by IB) without knowledge of diagnosis. Obtained score depended upon the amount of questioning required to elicit awareness of the perceptual incongruity and the clarity with which awareness was expressed, using the following rating scale:

0) No evidence of any recognition of incongruity after extensive interrogation.

1) Vaguely expressed recognition only after being asked where the back of the building was.

2) Recognition expressed only after being asked where the back of the building was.

3) Clearly expressed recognition only after being asked where the back of the building was.

4) Vaguely expressed recognition only after being asked if there was anything odd about the building.

5) Recognition expressed only after being asked if there was anything odd about the building.

6) Clearly expressed recognition only after being asked if there was anything odd about the building.

7) Vaguely expressed recognition only after being asked if there was anything odd about the picture.

8) Recognition expressed only after being asked if there was anything odd about the picture.

9) Clearly expressed recognition only after being asked if there was anything odd about the picture.

10) Spontaneous recognition of incongruity, vaguely expressed.

11) Spontaneous recognition of incongruity, expressed.

12) Spontaneous recognition of incongruity, clearly expressed.

Earlier inter-judge reliability checks with

TABLE 1

Analysis of Variance for Functional "Other," Depressed, and Brain-Damaged Subjects on Mean Recognition Scores

Source of Variance	SS	*df*	MS	*F*
Among Groups	152.18	2	76.09	5.56*
Within Groups	970.96	71	13.68	
Total	1123.14	73		

* $p = <.01$.

TABLE 2

Comparison among Groups with t*-ratio*

Groups	Mean	*df*	*t*	*p*
Functional "Others"	9.41			
vs. D		71	1.08	n.s.
vs. B. D.		71	3.33	<.01
Depressed (D)	8.29			
vs. B. D.		71	2.05	<.05
Brain-Damaged (B. D.)	5.95			

the same scorer had been .98 (3), so this step was not repeated.

RESULTS AND DISCUSSION

Analysis of variance was carried out to determine overall differences among groups (Table 1). With two and 71 degrees of freedom, respectively, the obtained *F*-ratio of 5.56 is significant beyond the .01 level of confidence. To examine the differences implied in the *F*-ratio, appropriate *t*-tests were done (Table 2), and the source of the difference was found to be between the Brain-Damaged and the other two groups. The comparison, Brain-Damaged vs. Functional "Others," yielded a probability value beyond the .01 level; Brain-Damaged vs. Depressed comparison gave a *p* value at the .05 level. The third comparison, Functional "Others" vs. Depressed, was not significant. Thus, our predictions were supported and, looking at the means for each group, we found that the ordinal relationship predicted was confirmed, although not every comparison reached statistical significance.

The question can be raised, especially when mean differences between Functional "Others" and Brain-Damaged are so great, whether heterogeneity of the populations themselves may have accounted for this difference. Hartley's test of homogeneity of variance does not support this view. The obtained *F*-ratio of 1.95 was not significant. Interestingly, the Brain-Damaged group contributed the maximum variance, the Functional "Others" the smallest.

With this question answered, it would appear that there is a real difference in identification of the perceptual incongruity among groups. Brain-damaged individuals, although they vary more than the other groups, show a decrement. To argue that they might never have had the ability to perceive implied three-dimensionality seems hardly reasonable. Much more reasonable would be the thesis that this impairment is a consequence of organic pathology. When we compare the mean score of the Brain-Damaged group with that of children (using data of Behar and Bevan), we find it to be of the same magnitude as that earned by the average five-year-old. To conclude that there is a perceptual regression to that developmental level is tempting, and this may possibly be the case, but the present findings alone do not warrant the conclusion. More study of the processes involved is needed.

To illustrate, we have taken the view that this was primarily a perceptual task. From the way *Ss* approached the task, there seemed to be no noticeable sensory impairment. However, examination of the scoring system, wherein both quality and clarity of response are variables in assigning scores, indicates clearly that at that level there are at least two factors involved: perceptual and expressive. Regarding the expressive, *Ss* may have been aware of the implied third dimension but unable to verbalize it. In the Behar and Bevan study with children, predominantly non-verbal estimates of intelligence were obtained (the Ammons Picture-Vocabulary Test and the Hildreth Puzzles). Their

reported lack of correlation between incongruity scores and IQ did not really answer the question of expressive differences. In our study, we attempted in a number of ways to deal with the problem. We found no differences among the groups in education, nor did we find any in reading comprehension, using the Ohio Literacy Test (8).[4] In both analyses of variance, the F-ratio was less than 1.0.

Full-scale Wechsler IQs were available for 48 *S*s. This analysis of variance among groups yielded an F-ratio that was not significant, despite a 12.5 IQ point difference favoring Depressives over Brain-Damaged. We compared the groups on Performance and Verbal separately, assuming that expressive differences should manifest themselves in the Verbal IQs. This was not the case. Differences significant at the .05 level were found on Performance, the Brain-Damaged scoring lower than either of the other groups. There were no significant differences in Verbal IQs. This lack of differences among groups was taken as evidence *against* the possibility that differential verbal expressive ability could account for the obtained result.

Differential ability in conceptualization may also have been involved in responding to the picture. Its effect could not be examined directly but, if *S*'s explanations of the incongruity could be used as a measure, could be estimated. It will be recalled that *S*'s first task was to identify the incongruity. After a response that could be scored was elicited, *S* was asked to provide a reason: "Why does the building look like it does?" and "How did it get that way?"

These qualitative responses were classified into a nine-category system. The lower the level of explanation, the conceptually more limited it was assumed to be. Thus, "No explanation" exemplifies Category 1; "Built that way to serve a purpose," Category 2; "Builder's mistake," Category 3; "Act of nature," Category 4; "Deliberate human act," Category 5; "Unfinished construction," Category 6; "Façade," Category 7; "Poorly or purposely misrepresented," Category 8; "Triangular building," Category 9.

With this schema, the modal category for four-year-olds was 2; five to seven-year-olds, 4. In the present sample, the modal category for the three groups was 8 ("Poorly or purposely misrepresented"). There was no evidence of differences among groups in explanation, hence conceptual ability appeared to be equivalent.

The lack of differences attributable primarily to conceptual, expressive or sensory impairment strengthens the possibility of perceptual decrement. In particular, the ability to perceive three-dimensionality when implied in the two dimensions of a picture appears to be worthy of further investigation. It may be that changes are specific only to pictorial stimuli, and have little relevance for spatial visualization and orientation generally. Systematic study of the effect of perspective cues and other orienting variables with populations differing developmentally seems indicated.

SUMMARY

Seventy-four subjects, divided into three groups (brain-damaged plus two comparison groups), were shown the photograph of a three-sided building, taken so that only the front was visible, and giving the impression of a two-dimensional façade. A reliable, structured interview established awareness of the perceptual incongruity, with the interview scored on the basis of the number of cues required to elicit correct verbalization of the incongruity.

Results supported the prediction that brain-damaged persons would be relatively more impaired in ability to perceive the pictorial representation of three-dimensional space than would the comparison groups.

[4] This test is administered routinely to establish reading comprehension level, as our population tends to be poorly educated.

Findings were discussed relative to the psychologic processes involved.

REFERENCES

1. Bay, E. Disturbances of visual perception and their examination. Brain, **76**: 515–550, 1953.
2. Becker, W. C. The process-reactive distinction: A key to the problem of schizophrenia? J. Nerv. Ment. Dis., **129**: 442–449, 1959.
3. Behar, I. and Bevan, W. The perception of incongruity by young children. Acta Psychol., **12**: 342–348, 1956.
4. Behar, I. and Bevan, W. The perception of incongruity by young adults. Paper read at Southern Society for Philosophy and Psychology, Atlanta, 1957.
5. Boger, J. H. An experimental study of the effects of perceptual training on group IQ test scores. J. Educ. Res., **46**: 43–52, 1952.
6. Ettlinger, G., Warrington, E. and Zangwill, O. L. A further study of visual-spatial agnosia. Brain, **80**: 335–361, 1957.
7. Forgus, R. H. The effect of early perceptual learning on the behavioral organization of adult rats. J. Comp. Physiol. Psychol., **47**: 331–336, 1954.
8. Foster, V. H. and Goddard, H. H. The Ohio Literacy Test. Pedagog. Semin., **31**: 330–351, 1924.
9. Goldstein, K. The effect of brain damage on the personality. Psychiatry, **15**: 245–260, 1952.
10. Gordon, O. J. and Tikofsky, R. S. Performance of brain-damaged subjects on Gottschaldt's Embedded Figures. Percept. Mot. Skills, **12**: 179–185, 1961.
11. Klüver, H. Visual disturbances after cerebral lesions. Psychol. Bull., **24**: 316–358, 1927.
12. McFie, J. and Zangwill, O. L. Visual-constructive disabilities associated with lesions of the left cerebral hemisphere. Brain, **83**: 243–260, 1960.
13. Newland, T. E. and Lawrence, W. C. Chicago Non-Verbal Examination results on an East Tennessee Negro population. J. Clin. Psychol., **9**: 44–46, 1953.
14. Piercy, M., Hecaen, H. and de Ajuriaguerra, J. Constructional apraxia associated with unilateral cerebral lesions—left and right sided cases compared. Brain, **83**: 225–242, 1960.
15. Rabin, A. I. and King, G. F. Psychological studies. In Bellak, L., ed. *Schizophrenia*, pp. 216–278. Logos Press, New York, 1958.
16. Updegraff, R. The visual perception of distance in young children and adults: A comparative study. University of Iowa Studies in Child Welfare, **4**: No. 4, 1930.

RESEARCH AS THERAPY?

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Increasing realization of the widespread incidence of mental disorder and various maladaptive behaviors has led to a demand for preventive techniques. Attention has slowly shifted from the pathologic individual to the treatment and study of his milieu. The institutional framework is no longer merely the context within which the individual's personality must be understood —it has become the focus for manipulations intended to have far-reaching consequences for whole groups of individuals and classes of behavior.

This paper discusses one of the problems involved in conducting research on preventive therapeutic techniques applied within closed social systems. In the course of evaluating a new program to combat military delinquency, the experimenter not only affected the object of his study but also was instrumental in its rapid disappearance. In addition to methodologic considerations, this finding will be discussed as an example of how the research investigator can free himself from the restricted role of participant observer and capitalize on these newly discovered powers.

This study was a successor to an earlier step in preventive social psychiatry, generated by the Army's concern with recidivism among military offenders. A program was developed in 1957 to have the Mental Hygiene Consultation Services (MHCS) screen psychiatrically all new prisoners at the various stockades and disciplinary barracks. Supposedly, this closer collaboration between disciplinary and mental health agencies produced new corrective techniques, in addition to expeditious discharge of men with poor prognosis and/or multiple courts-martial. A decrease in the number of men incarcerated during the years following the adoption of this screening program was interpreted as evidence for the significance of the efforts by the MHCS. It was never clear, however, whether this accomplishment could be credited to the development of new corrective techniques, or was simply a relaxing of the procedures for eliminating recidivists from military prisons.

Nevertheless, such an apparent victory in the battle against military delinquency was sufficient encouragement for a new plan to emerge, this one aimed at preventing rather than merely treating the disciplinary problem. The substance of this new program was to institute thorough psychiatric screening at the time of the soldier's *first* court-martial, rather than wait until two or three courts-martial resulted in his confinement in a disciplinary facility. Thus the first court-martial would be recognized as a signal of marginal adjustment and a suitable occasion for the deployment of the mental hygiene team.

Although many military posts immediately adopted this First Court-Martial Program into their standard operating procedures, it was initiated on a trial basis for a six-month period at Fort Dix, New Jersey. The following research design was formulated to evaluate the merits of this new approach to preventing military delinquency. Soldiers receiving their first court-martial were to be randomly assigned to one of four groups. The first two groups would receive a complete diagnostic evaluation by the MHCS, including a social history, psychologic tests and a psychiatric

[1] This research was conducted when the author was with the Clinical Psychology Section, Walson Army Hospital, Fort Dix, New Jersey. Present address: The Langley Porter Neuropsychiatric Institute, San Francisco, California. Appreciation is expressed to Mr. William Fisher, who assisted in collecting the data.

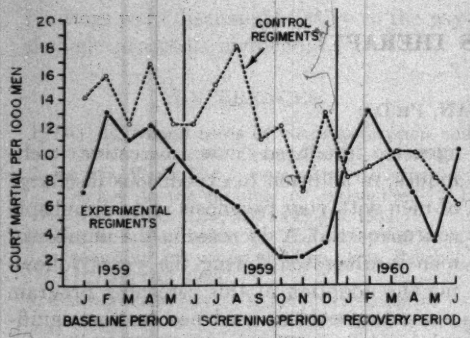

FIG. 1. Effect of screening program on courts-martial.

interview. MHCS conclusions and recommendations would be withheld from the referring source for the men in group 1, and sent back to the unit commanders for the subjects in group 2. Men assigned to group 3 would not actually be seen at MHCS, but would serve as a no-treatment control group. Group 4 would receive only a brief battery of psychologic tests (to provide information about the impact of being sent to a mental hygiene clinic), and would have minimal interpersonal contact. The subsequent military career of all of the subjects was to be studied for one year to evaluate the accuracy of the concerted clinical predictions.

To establish such a project within a closed social system, prudence indicated inclusion of some measure of the changes which might result from institutionalized feedback. Simply initiating a new program of any kind might—directly or indirectly—lead to some alteration in the typical behavior of the key members of the institution. This seemed particularly probable since the program entailed psychiatric screening, required the co-operation of quasi-legal and judicial authorities, and was to be performed within a military community. Subjects for the experiment were therefore drawn from only two out of the four training regiments at Fort Dix. The remaining two regiments served as additional institutional controls. In May, 1959, the unit commanders in the two experimental regiments were notified that, for the following six months, all first court-martial offenders must routinely be sent for evaluation to MHCS after completion of judicial proceedings. A directive signed by the commanding general was circulated in the experimental regiments in June, 1959, when the program actually began. No information was given to the personnel in the two control regiments. In December, 1959, the experimental regiments were informed that the trial period was over, and they need no longer send their first court-martial offenders to MHCS.

The principal finding of this study was that, immediately after the research project was started, the experimental regiments showed a marked drop in courts-martial but the control regiments remained unaffected. Consequently, there are no results to report on the four separate treatment groups, or on the accuracy of the clinical predictions, since very few first court-martial offenders were ever sent to MHCS during the six-month trial period.

Figure 1 gives the total courts-martial per 1000 men for the experimental and control regiments for the period January, 1959, to June, 1960—six months prior, six months during, and six months after the research project.

At the end of the research project the courts-martial given by the experimental regiments returned to the level of the controls. The courts-martial rate for the experimental and control regiments was tabulated for the previous year to check for seasonal variation: no systematic difference was found in the courts-martial rate during 1958 between the experimental and the control regiments.

Figure 1 shows a drop in courts-martial in both the experimental and the control regiments three months after the end of the research project (in April, 1960). Since this decrease in courts-martial seemed to pa-

allel directly the results obtained by six months of intense scientific study, an attempt was made to determine the possible antecedents. Informal communication revealed that there had been a meeting of unit commanders from all of the regiments with the legal judicial military agency early in April. The high courts-martial rate at Fort Dix was discussed, as was the recommendation that the parties concerned make more use of company punishments, rather than courts-martial.

Clearly, the program of psychiatric screening had a tremendous influence on the courts-martial behavior of the unit commanders. The most likely explanation is that this program displeased them, and they stopped giving courts-martial to prevent MHCS from making evaluations of the men they were punishing. While obviously there can be no direct evidence for such an interpretation, it seems plausible, especially since a similar drop in courts-martial occurred three months after the end of the research project, when all of the unit commanders were informed that their courts-martial rate was conspicuously high. Interestingly, the experimental regiments showed the most rapid drop in courts-martial following this meeting (April, 1960), whereas control regiments were not so easily influenced. Perhaps this difference reflects a sensitivity to environmental pressures which the experimental regiments had developed through their participation in the First Court-Martial Research Project.

The results obtained in this research study dramatically emphasize the need to control for institutional feedback in the conduct of research within a closed social system. Without the information provided by such controls, new ventures in preventive social psychiatry may have the appearance of curing a problem, whereas actually key members of the institution have merely changed their administrative procedures, or their specific techniques for identifying or handling a particular type of problem. If there had not been control regiments, or if the First Court-Martial Screening Program had not been established on a trial basis, it might well have seemed that the problem of military delinquency had been substantially reduced by the psychiatric screening. Such an interpretation assumes that the criterion used to measure a particular phenomenon is identical with the behavior itself; in actuality, courts-martial are not a problem, but only one index to the presence of the problem of military delinquency. A similar type of confusion between an index and the actual behavior may also be operative when a drop in prison population is interpreted as a tribute to the successful corrective efforts of the prison personnel, or when an increase in discharge rates in a mental hospital following a new treatment program is considered to reflect greater health in the patients. While the number of courts-martial, and the size of prison or hospital populations, are all operational definitions, they also, by definition, intimately involve major administrative decisions; as such, they may as well reflect a change in the mood of the bureaucratic structure, or pressures being brought to bear upon it, as any possible change in the behavior which these criteria supposedly measure. Thus, rapid increases in discharges in a mental hospital do not necessarily mean that the patients are getting well faster, or that more of them are getting well, but simply that some new factor has led to a change in the operation of the administrative procedures governing discharges. The essential element in the measure or control for changes in the operforming research, can be just such a factor, unwittingly exerting a radical influence on the typical behavior of the key members of an institution. Unless he provides some measure of control for changes in the operation of the institution itself, *E* may mistakenly conclude that he has witnessed an important change in the patient population he is ostensibly studying.

In a less serious vein, the investigation at Fort Dix might also suggest a new stratagem in our therapeutic armamentarium. Undesirable behavior of a social nature can be eliminated through the adoption of an elaborated program of research. Figure 1, showing the immediate increase in courts-martial in January, 1960, suggests that relentless research is necessary to keep down the court-martial response because of the rapid dissipation of inhibition. However, there may be some optimal procedure—such that the major portion of the research effort can go into planning and publicizing the research, with little or no experimental treatment of subjects actually necessary. The most economical scheme could be based upon present knowledge of partial reinforcement schedules. The institution might simply be informed that, at some time in the future, a full-blown research project may be initiated to study a particular problem.

Thus, the discovery of the Fort Dix phenomenon may serve to bridge the gap between the opposing camps of the experimentalists and the clinicians, through the emergence of a new professional personage: the therapeutic scientist.

ON THE GREATER AMPLIFYING POWER OF CAUSAL-CORRELATIVE OVER INTERROGATIVE INPUTS ON FREE ASSOCIATION IN AN EXPERIMENTAL PSYCHOANALYTIC SITUATION[1]

KENNETH MARK COLBY, M.D.[2]

INTRODUCTION

The general aim of this study was to investigate ways in which persons respond to two classes of verbal statements by another person under certain conditions. The specific hypothesis to be tested was that, in an experimental psychoanalytic situation, causal-correlative inputs have greater amplifying power on free association than do interrogative inputs. The investigation was concerned with aspects of the semantic and effectiveness problems of information theory.

If we picture spoken input as a vector, we find it has several components which can be measured and compared. Clinical experience results in the reasonable impression that not all spoken interventions on the part of an analyst are of equal power or significance. But power to do what? The task, shared by analyst and patient, consists of maximizing the patient's information about and understanding of those unrecognized beliefs which govern his behavior. The basic working tool for this task consists of free association, in which a patient attempts to describe inclusively his self-observation. The analyst attempts to catalyze the process of gaining access to unrecognized beliefs by selective spoken inputs. If we could classify and rank order inputs used by an analyst, an input with greater power could be defined as one which more frequently amplifies free association.

An experimental attempt was made to compare the amplifying effects of two classes of inputs on experimental subjects. Amplification was defined as both increasing the size of a response set and increasing the frequency of certain elements within that set. It was found that causal-correlative inputs have a significantly greater amplifying power than do interrogative inputs.

METHODS

SUBJECTS

The subjects were four unmarried male college students, ranging in age from 19 to 22 years. They were selected by someone other than the experimenter, in the following manner: A dean's office kept a list of students looking for work. When *E* was ready for a *S* he notified the office, and the dean or his secretary offered the job opportunity to the student whose name was next on the list. The dean's office knew only that the work involved psychologic experiments on free association. *S* was then seen by *E* for about five minutes to explain the nature of the work. *S*s were paid $5.00 an hour. The experimental design involved use of self-match controls, in which each *S* served as his own control.

PROCEDURES

The experimental setting was a large study in the wing of an old house situated in a suburban village. *S*s came four times a week for three weeks, each session lasting 30 minutes. Ten minutes of the final session were used to interview *S* as to his thoughts and feelings about the experiment. *S* lay on a couch behind which *E* sat. In the brief initial meeting, and again in the first experimental session, *S* was instructed

[1] This investigation was supported by a grant from Foundations' Fund for Research in Psychiatry.

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to think out loud and to describe as freely as possible, without rejecting, anything and everything which came to mind. The sessions were all tape-recorded, and Ss were guaranteed confidentiality.

Except for the usual hellos and goodbyes, *E* limited his statements to interrogatives and causal-correlatives which referred to immediate antecedents in the inclusive descriptions of *S*. After five or more sentences had been uttered by *S* about a significant person in his life (personage), one of the two inputs was made by *E*. An earlier study had indicated that imagoes—sets of propositions relating to personages—were relevant and manipulable variables in free association (1). With the use of ordinary clinical skills, the inputs were phrased so as to be unambiguous in intent and informational content.

Each input contained as referents a topic and a personage. The intent of an interrogative input was to elicit information regarding a topic and a personage appearing in the immediately antecedent associational flow. The intent of a causal-correlative input was to offer information about a topic and a personage appearing in immediately antecedent utterances. Grammatically, the interrogative consisted of a simple question. The causal-correlative consisted of a compound statement with two clauses linked by the conjunctive "because," the second clause containing a proposition inferred to be unrecognized, or only partially recognized, by *S*. Both were uttered by *E* matter-of-factly in his usual speaking voice, using appropriate vocal modifiers for each type of input. Examples follow:

Interrogatives:

Do your parents object to the marriage?

What did your father have against you?

What is the matter with your father's health?

What does she think of the Navy?

Causal-Correlatives:

You can't concentrate because you're worried she might find another guy.

You feel the room is shaking because this guy shakes you up more than you realize.

You think of this couple because you would like to have that sort of power over Julie.

Your father's wheezing bothers you because you are afraid he might die now.

After an input *E* remained silent for 10 to 25 minutes to allow free association to proceed undisturbed by further input. An example of antecedent associations, input and subsequent associations follows:

S: Frank borrows his uncle's car. He's a very poor dresser. I knew he doesn't have a lot of money. But neither does anybody. Dresses very sloppy and dirty. Clothes he wears over and over. And I know a lot of people who don't have as much money as he does but at least they look neat. He doesn't care about his appearance. He gets mad at people that dress up. When they even look nice, he says it's just a big front you're putting on. I feel like I'm shaking. As if . . . is the room shaking? Feels like it's shaking.

E: You feel the room is shaking because this guy shakes you up more than you realize.

S: Well, I was thinking . . . Yeah. I figure he actually has that kind of effect on me. I mean . . . I guess subconsciously because I don't think . . . Well, I can't remember when I've thought about it outside. Well, look—here's the thing. The guy, he's not big enough. I'd be actually scared to go and talk to him because the guy would really be offended. And I don't really want to hurt the guy's feelings. I mean, he hasn't done anything. But I'd like to go and talk to him. I'm mad. What can I say? Keep away from my girl? You know, this sounds like presumptuous, possessive. But she feels

the same way I do. She doesn't want him around.

SCORING

After the experimental sessions of each *S* the tapes were listened to several times and responses following input measured. Scoring was done while *E* was listening to tapes, not from a typescript. Verbal responses were divided into sentences following rules suggested by Dollard and Auld (2). The unit of scoring was the sentence, a single, minimum, free utterance. Each of *S*'s sentences following an input was scored on a lined tally sheet for the presence or absence of 1) the personage indicated in the input, and 2) the topic indicated in the input. A silence of five seconds was counted as a sentence empty of personage and topic. The response set was considered to last until five sentences occurred in which neither personage nor topic appeared. This arbitrary end-point of five empty sentences was arrived at through empirical experience, which showed that, in the great majority of instances, if both topic and personage dropped out of associations for five sentences, they were not returned to in subsequent associations. The response set thus consisted of all sentences following input up to and including the five blanks of the drop-out terminus. In this set, those particular sentences containing personage or topic, or both, were scored as tallies, those with neither as blanks. The size of the set and the percentage of tallies in the set were used as a measure of the amplifying power of the input. Thus an input followed by a set of 50 sentences and a tally score of 72 per cent was considered as of greater amplifying power than an input followed by a set of 20 sentences and a tally score of 30 per cent. Intonational patterns following input were not evaluated. Also, the alterative power of inputs was not measured in this study. Only the response set as defined and amplifying power were investigated. What occurred after the drop-out terminus was not pursued here.

RESULTS

The hypothesis that, in an experimental psychoanalytic situation, causal-correlative inputs have greater amplifying power on free association than do interrogative inputs tended to be confirmed.

Responses to inputs can be divided into two general classes: 1) proceedant, and 2) avoidant. In a proceedant response the topic or personage of the input is extended and amplified upon. In an avoidant response these elements are quickly dropped or do not appear at all, or the input is even met with silence. In this experiment, proceedance with a topic or personage indicated in the input was defined as a set in which the frequency tally was 15 per cent or more. An avoidant response consisted of less than 15 per cent tallies. The majority of the responses were proceedant (77 per cent). When avoidance occurred, it usually involved the topic (82 per cent) rather than the personage.

If the response set is taken to include either topic or personage, or both, the responses after interrogative and causal-correlative inputs differ (Figure 1). The response sets after interrogatives varied in length from nine to 34 sentences, with a mean of 19.1. The response sets after causal correlatives varied from 12 to 67 sentences, with a mean of 26.1. The mean percentage frequency of tallies after interrogatives was 49.8; after causal correlatives, it was 58.4.

Table 1 shows the number of each class of input given to each *S* and the mean number of sentences in each *S*'s response set. The differences between interrogatives and causal-correlatives are significant at the .05 level, using the *t* test. Table 2 shows the mean percentage frequency of tallies in the response sets of each *S*. These differences are significant below the .01 level, using the *t* test.

Each *S* varied in his individual responses

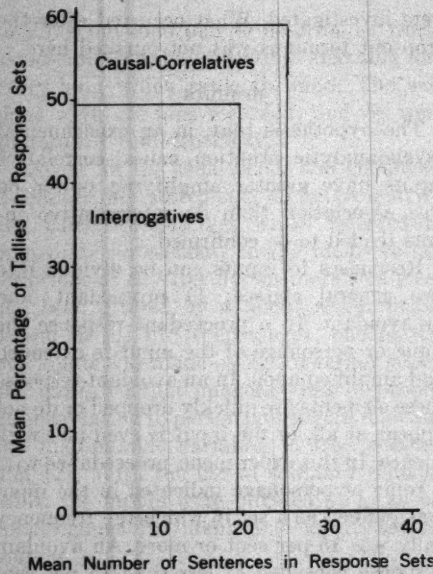

FIG. 1. Relation between mean number of sentences and mean percentage of tallies in response sets following interrogative and causal-correlative inputs.

to the two inputs, but almost consistently showed a more amplified response to a causal-correlative. No progressive effects were noted. When interviewed in the final session, all four *S*s selected a causal-correlative as having had the strongest effect on them.

A few general observations may be of interest. All *S*s reported having enjoyed the experience and having learned something from it. Three of the four felt that it had been helpful; the fourth did not. During the experiment one *S* was able to make an important decision in his life, which he attributed to hearing himself talking about and clarifying his problem. In the final interview period, all reported being aware of what they had talked about the most: family, male and female friends, and school. Also, they were aware that the inputs of *E* concerned persons close to them. Each had a somewhat different notion as to what the experiment was about. Each described himself as censoring different topics. All expressed a wish that *E* had spoken more (the complaint heard so often in clinical discourse).

DISCUSSION

EXPERIMENTAL CONTEXT

Some conditions of this experimental situation should be considered before the nature of these two classes of inputs and tentative reasons for their effects are discussed.

The arrangement of the physical setting in which the experiment took place was easily inferred to be the working area of a psychologist, psychiatrist or psychoanalyst, and *E* was known to be a "doctor." The social context between *E* and *S* involved a work contract, not a therapeutic one. *S* was paid rather high wages to do his best to describe inclusively his self-observation. Inevitably, in such a situation, psychotherapeutic factors enter. The relationship between *E* and *S* becomes then a mixture of working collaborative and transference-countertransference elements. While *S*s made occasional references to *E* in their flow of associations, he did not make any reference to them in his inputs.

"MEANTENT"

In this experimental context, then, the central questions for discussion are, Why did *S*s respond as they did to *E*'s inputs? Why were the size and tally density of the response sets greater in the case of causal-correlative input? To answer these questions, the meaning of spoken input to *E* and to *S* must be reflected upon. I will define the meaning of an utterance in two-person discourse by the following pentadic formula (adapted from Leonard (3)) and term it *meantent:*

$$M = I^{t}\ (s,\ f\ (r,\ p))$$

Meantent (M) is equivalent to the intent (I) of a speaker (s) at a certain time (t)

that a receiver (r) stands in relation (f) to a certain proposition (p). A proposition is considered to be any state of affairs referred to by an utterance. The relation in which a speaker intends a receiver to stand toward a proposition consists of, in the case of an interrogative, "find answer," and in the case of a causal-correlative, "accept-reject." For every utterance there are two meantent sources, one encoded by a speaker and one decoded by a receiver. The semantic problem of information theory concerns itself with the degree of correspondence between encoded and decoded meantents.

Suppose *E* utters the interrogative: "What did your father have against you?" The encoded meantent is: My intent at this time is to request him to describe more about his father's antagonism. If a perfect correspondence in the decoded meantent occurred, *S* would understand that he is being requested to say more about the topic of his father's antagonism to him. The encoded meantent can be compared to a computer instruction. It states, "Do this to that"—namely, carry out a certain operation on a certain operand.

There can be several decodings of a given utterance, including the perfect fit mentioned above. A common one of these consists of an inference on the part of the receiver regarding an additional intent of the speaker. That is, the receiver grasps not only the first level of intent (*e.g., what* the speaker is asking), but also wonders about a second level of intent (*e.g., why* is he asking this). When several levels of intent are pursued by the receiver, a chained sequence of decodings takes place. In principle, there is a potentially infinite series of decodings for any utterance if the receiver has sufficient time and desire to construct them.

In the case of a simple interrogative, the encoded meantent instruction specifies a request for certain information. Congruent decoding by *S* involves his understanding this instruction. More complex is the meantent instruction of a causal-correlative input. The proposition contained in the second clause of a causal-correlative is inferred by *E* to be unrecognized or only partially recognized by *S*. While an interrogative is designed to elicit information, a causal-correlative is designed to offer information new to the receiver's awareness. The encoded meantent of a causal-correlative input is: My intent at this time is to inform you that you feel this because of that. The decoded meantent at the first level involves an understanding of the speaker's intent to offer a causal proposition for the receiver's consideration.

TABLE 1

Number of Inputs and Mean Number of Sentences in Response Sets Following Interrogatives and Causal-Correlatives

Subject	Interrogatives		Causal-Correlatives	
	No. of Inputs	Mean No. of Sentences	No. of Inputs	Mean No. of Sentences
1	8	13.8	7	19.0
2	7	24.5	3	37.0
3	8	19.5	4	21.7
4	6	18.6	5	26.8
Total	29	76.4	19	104.5

$t = 3.19$, $p = .05$.

TABLE 2

Mean Percentage of Tallies in Response Sets Following Interrogative and Causal-Correlative Inputs

Subject	Interrogatives	Causal-Correlatives
1	44.3	53.4
2	56.5	63.3
3	53.0	59.7
4	44.8	57.4
Total	198.6	233.8

$t = 6.38$, $p < .01$.

RESPONSE TO MEANTENT

Once the meantent of an input is decoded, the receiver selects a response. But why does he respond at all? We pause here

only to remark on the programable nature of persons in two-person discourse. When asked a question, a person usually responds. In addition, an experimental *S* feels that responding is part of the work he is being paid to do. When offered a causal-correlation in this experimental situation and by this sort of expert, *S*s feel that a response is called for and act accordingly.

A causal-correlative input carries an implicit instruction first to confirm or infirm the truth of the proposition offered. (It must be remembered that the truth in question here is always that of privately felt truth, and not that of publicly demonstrated truth.) The usual response is to declare acceptance or rejection of the truth of a causal-correlation, and then to elaborate with an extension of the selected response.

For an experimental *S*, a causal-correlative input is more ambiguous as to what response to select, once he is beyond an immediate agreement or disagreement. What direction to develop and when to terminate are less clearly defined than in the case of an interrogative.

Further properties of the two inputs deserve comparison. Both types of inputs were designed to refer to *S*'s imago system, a set of propositions involving significant other persons in his life. Thus they referred to states of affairs of equivalent import. The inputs differ, however, in information as surprise. Since the proposition following the conjunctive "because" has been unrecognized, a causal-correlative input contains a surprise value. The information is unexpected, and hence the amount of information conveyed is greater, as measured roughly by our intuition that the greater the surprise to a receiver, the more information he has received. Surprise results not simply from hearing the input, but also from the receiver's confirming the causal-correlation by self-observation. In this response, not only are the two clauses accepted as true, but also their conjunctive relation is recognized as a causal one. Since it concerns a motive and offers a hy-explanation, a causal-correlation offers potentially useful information for a person's self-corrective change. This input indicates both that which is attention-worthy and that which is potentially change-worthy. In decoding a causal-correlative, *S* may feel it is being offered to help or benefit him.

Thus differences in three properties of these inputs may account for the differences in their amplifying power. They differ in meantent instruction, in information as surprise, and in their utility value for psychologic benefit.

While the experiment was of a basic science nature, perhaps a few remarks should be made about clinical implications. As mentioned, the goal of psychoanalytic treatment is an increase in a patient's information about and understanding of those unrecognized beliefs or fantasies which determine detrimental behavior patterns. Access is gained to these beliefs by the process of free association, aided by inputs designed to facilitate access. An increasing proceedance toward them and a progressive reduction of avoidance are necessary. Inputs of the therapist, while designed to facilitate free association and to minimize avoidance, inevitably arouse some degree of impedance, since the areas being approached are highly conflictual. The experiment suggests, not that interrogatives are ineffective, but that causal-correlatives are significantly more effective in amplifying free association. Because of this greater amplifying power, an increased use of causal-correlative inputs might represent a more effective heuristic in clinical discourse. Other sorts of inputs, with properties similar to causal-correlatives, may be of equal or greater amplifying power. Further studies will be necessary to test this possibility and to establish whether the properties considered are necessary or sufficient conditions

for maximum amplification of free association.

SUMMARY

Two classes of inputs, interrogative and causal-correlative, were compared for their amplifying power on free association of four subjects in an experimental psychoanalytic situation. Causal-correlative inputs showed a significantly greater amplifying power. Tentative reasons for the inequality of effect were discussed and a clinical implication was noted.

REFERENCES

1. COLBY, K. M. Experiment on the effects of an observer's presence on the imago system during psychoanalytic free-association. Behav. Sci., **5**: 216–232, 1960.
2. DOLLARD, J. AND AULD, F. *Scoring Human Motives: A Manual*, pp. 12–18. Yale University Press, New Haven, 1959.
3. LEONARD, H. S. Authorship and purpose. Phil. Sci., **26**: 277–294, 1959.

LIBRIUM THERAPY AND ELECTROENCEPHALOGRAPHIC CORRELATES

DON L. WINFIELD, PH.D.[1] AND G. H. AIVAZIAN, M.D.[1]

In an earlier study (19) chlordiazepoxide (Librium[2]) was evaluated as a sedative agent in preparing a patient for an electroencephalogram (EEG). Used in a series of 226 consecutive referrals of severely disturbed patients, Librium proved to be highly effective, inducing sleep or drowsy relaxation in 90 per cent. Only two EEGs were technically unsatisfactory.

The efficacy and safety of Librium used primarily as a relaxant, the observations made during this study, and the by now extensive literature on the use of Librium in psychiatry and related fields (6) prompted further study. The present report is concerned mainly with possible EEG correlates with the variables encountered in the treatment of psychiatric patients and with changes in the EEG induced by Librium.

Since the advent of the psychotropic drugs, many studies of their effects on the EEG have been necessary to determine, in evaluating the recordings, the extent of the chemically induced modifications. The characteristic patterns and other effects of the barbiturates, meprobamate and various phenothiazines (2, 5, 7, 20) are familiar to the electroencephalographer. The "Librium effect," first described by Kaim (8), and reported in greater detail by Towler (17), occurs in patients taking the drug in therapeutic doses. As a rule, such patients develop low voltage fast frequencies tending to displace or to obscure the normal alpha background activity (Figure 1). In a highly interesting report, Turner and his colleagues (18) noted the tendency of daily Librium doses of 30 to 60 mg. by mouth to normalize abnormal EEGs, replacing irregular, disorganized slow waves with better organized, faster activity and higher voltage. In a study comprising more than 70 subjects, these authors observed the disappearance of a variety of abnormal patterns in epileptics. They found in subjects with petit mal seizures that not only did the 3 per second wave and spike pattern disappear, to be replaced by more normal activity, but also the seizures were better controlled. The potential role of Librium as an anticonvulsant, first suggested by Kaim and Rosenstein (8), is now under the necessary long-term investigation (9, 13).

In an earlier study the Librium effect was not encountered, since it occurs only after the patient has been under medication for several days. The objectives of this study were to observe this effect, to note other changes in the EEG itself, and to investigate any correlations between clinical and EEG effects as part of a general study of the therapeutic use of Librium in a group of psychiatric patients. Some of these patients had abnormal EEGs and/or a history of presumed cerebral insult, with or without symptoms that may have been related to the central nervous system. In a group including many adolescents with severe behavior disorders, and patients with anti-social records or tendencies, the potentialities of Librium to affect the central nervous system would be of high social as well as medical interest. The specific site or sites of action of this drug have still to be established, and perhaps the study of its influence on certain EEG patterns at least tentatively associated with certain areas of the brain may help us to understand where and how this compound does its work.

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[2] Trademark of Hoffmann-LaRoche Inc., Nutley, New Jersey, for chlordiazepoxide.

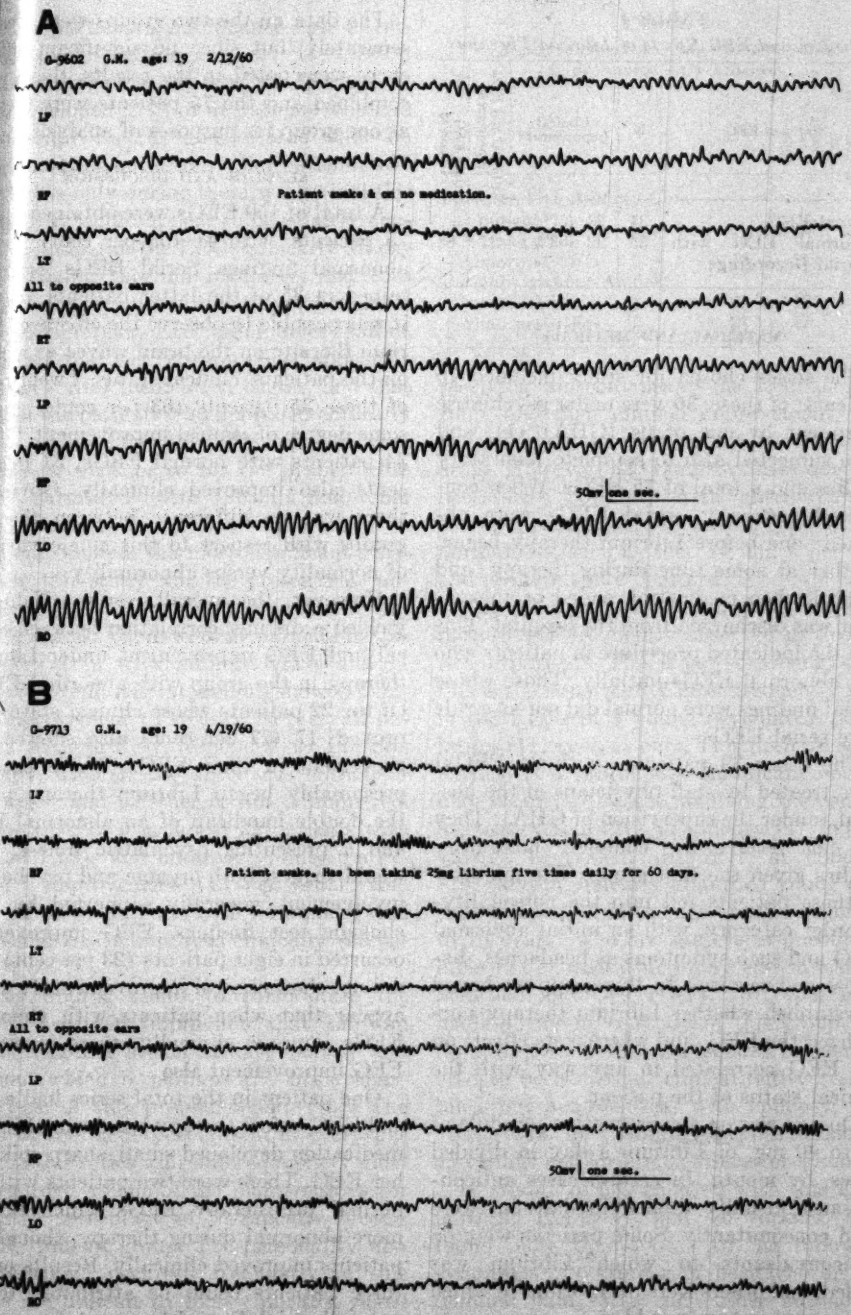

FIG. 1. Sample EEG tracings to demonstrate Librium effect. A: Before treatment. B. During treatment with Librium.

TABLE 1

Clinical and EEG Effects of Librium Therapy

Status of EEG	N	Clinical Improvement	Clinical and EEG Improvement	Only EEG Improved
Normal EEG	34	23 (67%)		
Abnormal EEG with Serial Recordings	35	22 (63%)	17	8

MATERIAL AND METHODS

The series chosen for study included 75 patients; of these, 36 were under psychiatric treatment by one of us (GHA) (1), and were subjected also to complete laboratory studies and a total of 75 EEGs. When considered necessary, serial EEGs were obtained: one before Librium therapy began, another at some time during therapy, and a third when medication ended or the patient was discharged from the hospital. This was the indicated procedure in patients who had abnormal EEGs initially. Those whose initial findings were normal did not as a rule have serial EEGs.

The other 39 patients (with 84 EEGs) were treated by staff physicians of the hospital (under the supervision of GHA). They did not receive the complete laboratory studies given the first group. The majority of these patients fell into the personality-disorder category, with an initial abnormal EEG and such symptoms as headaches, dizziness and seizures. In this group we hoped to establish whether Librium therapy normalized the EEG, and whether its effects on the EEG correlated in any way with the clinical status of the patient.

Dosage was on a conservative schedule of 30 to 40 mg. of Librium a day in divided doses, by mouth. In certain cases anticonvulsants, usually diphenylhydantoin, were used concomitantly. Some patients were on anticonvulsants to which Librium was added, and others were on Librium to which anticonvulsants were added.

The data on the two groups were treated separately, but since no significant differences were noted in the results, these were combined and the 75 patients were treated as one group for purposes of analysis.

RESULTS AND DISCUSSION

A total of 159 EEGs were obtained on the 75 patients, with 41 (55 per cent) having abnormal findings. Serial EEGs were obtained in 35 of the latter patients, so that it was possible to observe the effects of Librium therapy on the brain waves as well as on the patients' clinical status. Twenty-two of these 35 patients (63 per cent) showed some degree of clinical improvement. Of the 34 patients with normal EEGs, 23 (67 per cent) also improved clinically. Obviously there was no difference between the two groups with respect to this single variable of normality versus abnormality.

However, the overall results (Table 1) yielded a distinct correlation between clinical and EEG improvement under Librium therapy in the group with abnormal EEGs. Of the 22 patients whose clinical status improved, 17 (77 per cent) also showed improvement in their EEGs. These patients presumably began Librium therapy under the double handicap of an abnormal EEG and a presenting psychiatric illness. This would suggest both organic and psychologic involvement, generally supported by psychologic test findings. EEG improvement occurred in eight patients (23 per cent) who did not show clinical improvement. It would appear that when patients with abnormal EEGs improve clinically, there is usually EEG improvement also.

One patient in the total series had a normal EEG before therapy but during Librium medication developed small, sharp spikes in her EEG. There were two patients with abnormal pre-therapy EEGs which became more abnormal during therapy, though the patients improved clinically. Results on the three patients could be attributed to the normal variability of brain wave patterns,

and no particular significance can be attached to this finding.

Low voltage fast activity, the Librium effect, developed in 41 of 51 patients who had pre-therapy records and records while on Librium. Nine out of ten other patients who had EEGs only during therapy also had this pattern. It would appear to be a fairly consistent finding in the EEGs of nearly all patients taking Librium regularly, and thus it can be taken into account by the electroencephalographer. Like other investigators, we found that the Librium effect appears a few days after medication begins, and that most of the patients who improved clinically did so within a few days. This suggests that it takes a week or so for the brain to become "saturated" with Librium and relief of symptoms to begin.

In our opinion, the most significant variable in this study was the presence or absence of an anxiety component in the patient's illness. There were 21 patients in this category (ten with normal EEGs and 11 with abnormal EEGs), and all showed clinical improvement (Table 2). In fact, all patients with anxiety responded to Librium therapy, whether the EEG was normal or abnormal, and whether or not it improved. In contrast, of a group of 22 patients with normal EEGs and no anxiety, only ten exhibited improvement (43 per cent); and only 17 of 31 patients with no anxiety and abnormal EEGs improved clinically (55 per cent).

This finding seems to corroborate the widely accepted impression that Librium exerts a potent action against anxiety and tension, which is perhaps the basic characteristic of this protean agent (16). The relief of the anxiety symptom in every patient in this series who suffered anxiety must be regarded as one of the main reasons for the high percentage of therapeutic success in this difficult group. The personality disorders which occurred frequently in this series are difficult to treat clinically. Some of the patients with abnormal EEGs were receiving anticonvulsants as well as Librium, and their clinical improvement may be attributed in part to the use of these drugs. However, EEG improvement does not necessarily correlate with anticonvulsant therapy, particularly in certain types of abnormalities, such as the "14 and 6 per second positive spike" pattern.

TABLE 2

Significance of Anxiety Component in Improvement with Librium

	N	Clinical Improvement	Percentage Improved
Patients with Anxiety Component			
Normal EEG	10	10	100
Abnormal EEG	11	11	100
Patients without Anxiety			
Normal EEG	23	10	43
Abnormal EEG	31	17	55

The "14 and 6 per second positive spike" pattern was frequently observed in this series of patients. This is not surprising, since this is the abnormality most frequently seen in the EEGs of a young psychiatric population (11) such as ours. This positive spike pattern is a profoundly significant one. It may occur in diseases involving the central nervous system (21), in epilepsy (4), in convulsive equivalent disorders in children (10), and in violently disturbed young persons (almost all in the age group 18 months to 16 years) who are subject to uncontrollable fits of rage which may end in crimes. Schwade and Geiger (14, 15), who have made an intensive study of this group, believe that their disorder should be considered to be a distinct clinical entity resembling but not identical with epilepsy, and "the result of thalamic, hypothalamic or amygdalic and/or limbic disease or trauma."

In studies of 550 children with this EEG pattern, Kellaway and co-workers (10) found more than half with the following symptoms: 1) attacks of headache or abdominal pain, with such other autonomic disturbances as nausea, vomiting, dizziness

TABLE 3
Effect of Librium Therapy on Certain EEG Patterns

Pattern	N	EEG Improved	Clinical Improvement	Clinical and EEG Improvement
"14 and 6 per second Positive Spike"				
Patients with Serial EEGs	19	14 (74%)	12	12 (63%)
No Serial EEG	3		3	
	22		15 (68%)	
"5 to 6 per Second Wave and Spike Phantom"				
Patients with Serial EEGs	10	8 (80%)	6	6 (60%)
No Serial EEG	1		1	
	11		7 (64%)	

and temperature changes; 2) behavioral disturbances; 3) seizures, often atypical but including generalized convulsions. This pattern appears to be a specific EEG correlate of convulsive equivalent disorders in children.

Gibbs (4) has remarked that parents frequently refuse to take their children's complaints of pain and dizziness seriously enough. Since as a rule no obvious organic difficulty is observed in such children, many years of antisocial behavior may precede referral for an EEG that may reveal this abnormality. Therapy is often difficult in these patients, but with anticonvulsants, various psychosedatives and psychotherapy, some have a good prognosis.

In view of these considerations, the response to Librium of the group with this EEG pattern was of particular interest. Twenty-two patients showed the "14 and 6 per second positive spike" pattern in their EEGs (Table 3). Of these, three did not have a recording before Librium therapy was initiated. In the group of 19 with serial EEG studies, 14 (74 per cent) showed EEG improvement, and only two of them failed to improve clinically as well. All three patients who did not have serial EEGs improved clinically, making a total of 15 (68 per cent) who responded favorably to Librium therapy.

Of this group of 22 patients, seven received anticonvulsants in addition to Librium; EEG improvement was noted in six, although three failed to respond clinically. That the "14 and 6 per second positive spike" pattern responds well to anticonvulsants has been established; that Librium itself may have anticonvulsant properties may explain the success of this combined therapy. Investigators (8, 9, 13) who have used Librium as an anticonvulsant employed much larger doses than those scheduled in the present study. Results would certainly justify a more thorough trial with larger dosages of Librium, alone or in combination with other drugs, in patients with severe behavior disorders.

Another interesting abnormal EEG pattern is the "5 to 6 per second wave and spike phantom," which is essentially a one-second burst of wave and spike (12). Patients with such a pattern usually have a history of syncopal-like attacks, or they may have generalized seizures. In our study, 11 patients had this pattern (Table 3). One patient with only a pre-therapy recording improved clinically. Of the ten serial studies, eight showed improvement in the EEG, and six of the eight also showed clinical improvement. Two patients with improved EEGs were taking an anticonvulsant in addition to Librium; one improved clinically, and one did not. In this group of patients the role of anticonvulsants was not significant. It may well be that better results would have been obtained if a combination of drugs in maximal tolerated doses had been used. Though this group is relatively small, the tendency to normalize or improve the EEG in such a high percentage suggests that Librium, alone or in combination with

other drugs, might prove to be a specific for patients with this abnormality.

Any EEG study of Librium stimulates speculation as to whether it operates at a cortical level, a subcortical level, or both. Since the Librium effect of fast activity occurs in the waking state, it is possible that there is cortical involvement. The tendency of the drug to normalize or decrease the frequency of the "14 and 6 per second positive spike" pattern, which presumably originates in subcortical structures (3), would suggest that Librium works at subcortical levels also. Other evidence has been reported (18) that Librium normalizes EEGs with the classic 3 per second spike/wave pattern of true idiopathic petit mal epilepsy. Presumably this pattern is of subcortical origin. The broad-spectrum effectiveness of Librium may thus be accounted for, in part, on the assumption that it works at both cortical and subcortical levels.

SUMMARY AND CONCLUSIONS

Librium therapy, in a moderate dosage schedule of 30 to 40 mg. per day in divided doses, was used in a series of 75 psychiatric patients, including a group of severe behavior problem adolescents. A total of 159 EEGs was obtained; in 35 of the 41 patients with abnormal recordings, serial studies were done before, during and after Librium therapy. Of this group of 35, 25 showed EEG improvement, 22 improved clinically, and 17 improved both clinically and in the EEG. Of the 34 patients with normal EEGs, 23 had a satisfactory clinical response to Librium therapy.

All patients with an anxiety component in their illness improved clinically, whether the EEG was normal or abnormal, and whether or not it improved. Five of eight patients with anxiety and abnormal EEGs showed both clinical and EEG improvement.

Low voltage fast activity, the Librium effect, appeared in the EEGs of nearly all of the patients who had been on therapeutic doses of Librium for several days. Seemingly, clinical improvement and the appearance of the Librium effect generally coincide.

The favorable clinical results, and the tendency of the EEG to normalize or improve in patients with the "14 and 6 per second positive spike" and the "5 to 6 per second wave and spike phantom" patterns, are provocative and should be studied further. It is possible that higher dosage levels of Librium might be used to advantage in the more serious disorders.

Speculation as to the site or sites of Librium action was stimulated by observations made during this study. The Librium effect may operate on a cortical level, and the tendency of abnormal patterns presumed to be of subcortical origin to disappear or to become less frequent under Librium dosage suggests that this drug operates also at a subcortical level. This might help to explain the broad effectiveness of Librium in a variety of disorders.

REFERENCES

1. Aivazian, G. H. Clinical evaluation of chlordiazepoxide. J. Tenn. Med. Ass., **54**: 115–119, 1961.
2. Brazier, M. A. B. and Finesinger, J. E. Action of barbiturates on the cerebral cortex. A.M.A. Arch. Neurol. Psychiat., **53**: 51–58, 1945.
3. Gibbs, E. L. and Gibbs, F. A. Electroencephalographic evidence of thalamic and hypothalamic epilepsy. Neurology, **1**: 136–144, 1951.
4. Gibbs, F. A. Clinical correlates of 14 and 6 per second positive spike. Electroenceph. Clin. Neurophysiol., **8**: 148, 1958.
5. Henry, C. E. and Obrist, W. D. The effect of meprobamate on the electroencephalogram. J. Nerv. Ment. Dis., **126**: 268–271, 1956.
6. Hines, L. R. Methaminodiazepoxide (Librium): A psychotherapeutic drug. Curr. Ther. Res., **2**: 227–236, 1960.
7. Jörgensen, R. S. and Wulff, M. H. The effect of orally administered chlorpromazine on the EEG of man. Electroenceph. Clin. Neurophysiol., **10**: 325–329, 1958.
8. Kaim, S. C. and Rosenstein, I. N. Anticonvulsant properties of a new anticonvulsant drug. Dis. Nerv. Syst. (Suppl.), **21**: 46–48, 1960.
9. Kaim, S. C. and Rosenstein, I. N. Experience with chlordiazepoxide (Librium) in the man-

agement of epilepsy. J. Neuropsychiat. In press.
10. KELLAWAY, P., CRAWLEY, J. W. AND KAGAWA, N. A specific electroencephalographic correlate of convulsive equivalent disorders in children. J. Pediat., **55**: 582–592, 1959.
11. KNOTT, J. Comments at the American Electroencephalographic Society Meeting, Santa Fe, New Mexico, October, 1957.
12. MARSHALL, C. Some clinical correlates of the wave and spike phantom. Electroenceph. Clin. Neurophysiol., **7**: 633–636, 1955.
13. ROSENSTEIN, I. N. A new psychosedative (Librium) as an anticonvulsant in grand mal type convulsive seizures. Dis. Nerv. Syst. (Suppl.), **21**: 57–60, 1960.
14. SCHWADE, E. D. AND GEIGER, S. G. Severe behavior disorders with abnormal electroencephalograms. Dis. Nerv. Syst., **21**: 616–621, 1960.
15. SCHWADE, E. D. AND GEIGER, S. G. Matricide with electroencephalographic evidence of thalamic or hypothalamic disorder. Dis. Nerv. Syst., **14**: 18–20, 1953.
16. TOBIN, J. H. AND LEWIS, N. D. C. New psychotherapeutic agent, chlordiazepoxide: Use in the treatment of anxiety states and related symptoms. J. A. M. A., **174**: 1242–1249, 1960.
17. TOWLER, M. Librium: A preliminary report. Presented at the Southern Encephalographic Society Meeting, Louisville, Kentucky, November, 1960.
18. TURNER, W. J., KRUMHOLZ, W. V. AND MERLIS, S. The effects of Librium (methamindiazepoxide) (Ro 5-0690) on the human electroencephalogram. Presented at the American Encephalographic Society Meeting, Boston, June, 1960.
19. WINFIELD, D. L. The use of Librium in clinical electroencephalography. J. Neuropsychiat., **2**: 191–194, 1961.
20. WINFIELD, D. L. AND AIVAZIAN, G. H. EEG changes associated with intensive Sparine therapy. Electroenceph. Clin. Neurophysiol., **10**: 575, 1958.
21. WINFIELD, D. L., BRITT, L. P. AND RASKIND, R. EEG findings in pseudohypertrophic muscular dystrophy. Electroenceph. Clin. Neurophysiol. (prelim. report in Soc. Proc.), **10**: 203, 1958.

RELATIONSHIP OF FETISHISM AND TRANSVESTISM TO BRAIN AND PARTICULARLY TO TEMPORAL LOBE DYSFUNCTION

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Both fetishism and transvestism are characterized by the dominance of symbol or sign in sexual activity. In fetishism, arousal and orgasm are attained in a forced and stereotyped fashion by means of an object; other characteristic behavioral abnormalities are almost invariably present (4). Transvestism (cross-dressing) may assume varied forms (7) but is closely related to fetishism, particularly when articles of clothing evoke the imperative need that they be donned. This form of transvestism may properly be called fetishism-transvestism. In transvestism, the act of cross-dressing is not commonly followed by orgasm but rather by a sense of profound gratification.

It has recently been suggested that a disorder in brain mechanisms, primarily related to the temporal lobe, plays an etiologic role in the genesis of fetishism (4). Indeed, Mitchell *et al.* (8) described a patient with fetishism and temporal lobe seizures, both relieved by temporal lobectomy. Krafft-Ebing's case recordings (6) also indicate the occurrence of epilepsy in some fetishists.

Davies and Morgenstern (3) described a patient with transvestism which did not appear until middle life, and then only after the development of seizures of both grand mal and temporal lobe type. These authors listed three additional cases of transvestism with evidence of temporal lobe dysfunction, including one with a small astrocytoma in the right uncal region.

Petritzer and Foster (10) reported a case of transvestism in a 14-year-old boy with epilepsy and juvenile diabetes. Yawger (11) described a patient with apparent grand mal epilepsy in whom, along with various other behavioral abnormalities, "there has been the wish for women's clothes." In neither of these two cases was there evidence of focal involvement of the temporal area.

A related phenomenon has been described by Clark in his case studies on the psychosexuality of epileptics, published in 1915 (2). One of his patients (Case 3) developed epilepsy at the age of 19. The seizures were both grand mal and psychomotor in type; the epileptogenic focus was undoubtedly temporal in location, since the seizures began with characteristic stereotyped phenomena, including complex auditory and visual hallucinations. This patient "during the first three year period of his disease . . . gradually assumed many female characteristics, used powders and scents, decorated his room with photographs and pretty colored cloths . . ." This is an example of effemination in behavior occurring in an individual with temporal lobe dysfunction.

This paper presents five new patients. The first is a case of fetishism; the second and third, of fetishism-transvestism; the fourth, of transvestism; the fifth, of transitory fetishistic-transvestitic behavior. The findings in these patients support the hypothesis relating fetishism and transvestism to brain—particulary temporal lobe—dysfunction. In these brief case presentations, clinical and electroencephalographic data are emphasized; no attempt is made, because of the nature of the study, to state the pertinent psychodynamics.

CASE MATERIAL

Case 1: Fetishism

The patient was a 31-year-old unmarried college graduate with boot and raincoat fetishism. These fetish objects had marked condensation of meaning. The boot (a

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man's) symbolized the penis primarily, but it, as well as the raincoat, also represented the mother's body.

The patient displayed many of the characteristics described in fetishists (4): a reserved manner with character traits of the obsessive-compulsive variety; preoccupation with the fetish objects and their frequent involuntary appearance in awareness; the appearance of the boots and raincoat in dreams, usually accompanied by seminal emission; the domination of psychic life not only by the fetish but also by related objects or thoughts which assumed magical power; the sudden and involuntary appearance of thought fragments in consciousness, often associated with marked tension and discomfort; regular self-stimulation, particularly frequent during adolescence and early adulthood; attainment of orgasm by means of the fetish and related fantasies.

The patient had had an intense and ambivalent relationship with his mother from infancy. He was not erotically aroused by women and had never had coitus. Erotic arousal and sexual fantasies consisted of homosexual (in which he assumed the feminine role) and fetish motifs.

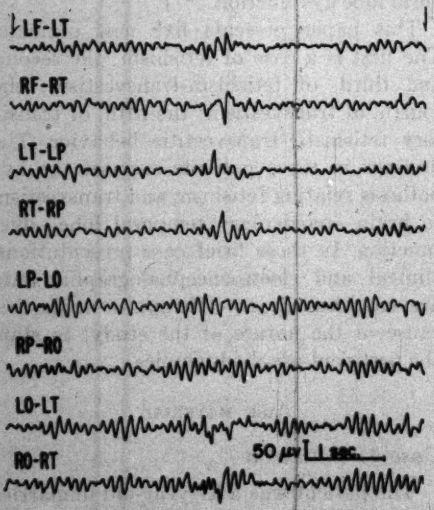

FIG. 1. Fragment of EEG record obtained from Case 1 after administration of Metrazol, 300 mg.

There was no history of epilepsy. During infancy and early childhood there was an unusual degree of motor hyperactivity. As far as can be determined, his preoccupation with the raincoat began at six years. The very powerful erotic feelings for men's boots developed at ten years. For a brief period at the age of nine or ten years he played with dolls, and occasionally put a girl's dress over his clothes.

An electroencephalogram showed spike forms over the right temporal electrode. Intravenous administration of 300 mg. of Metrazol produced bitemporal spiking, maximal on the right (Figure 1).

Case 2: Fetishism-Transvestism

The patient, a 53-year-old unmarried man confined to a state mental hospital, had almost always worn women's underpants under his clothes.

He recalled "wanting to be a girl" from the age of four or five. Somewhat later in childhood he developed a specific interest in "panties," but not until adult life did he begin to wear them constantly. He practiced self-stimulation, using the panties as a stimulus. He had accumulated approximately 200 pairs of panties, and kept a record of all he bought. He occasionally shaved the skin of his abdomen to appear more feminine. Since his institutionalization at the age of 46 the need to wear the underpants has diminished, although he recalls during this period at least one dream in which the panties appeared.

The patient had had grand mal epilepsy since the age of 15. Interestingly, no convulsions have occurred in the last seven years, during which time his need to wear the underpants has also diminished.

The patient was apparently able to make an adequate environmental adjustment until about the age of 44, when he developed uncontrollable crying episodes. Auditory hal-

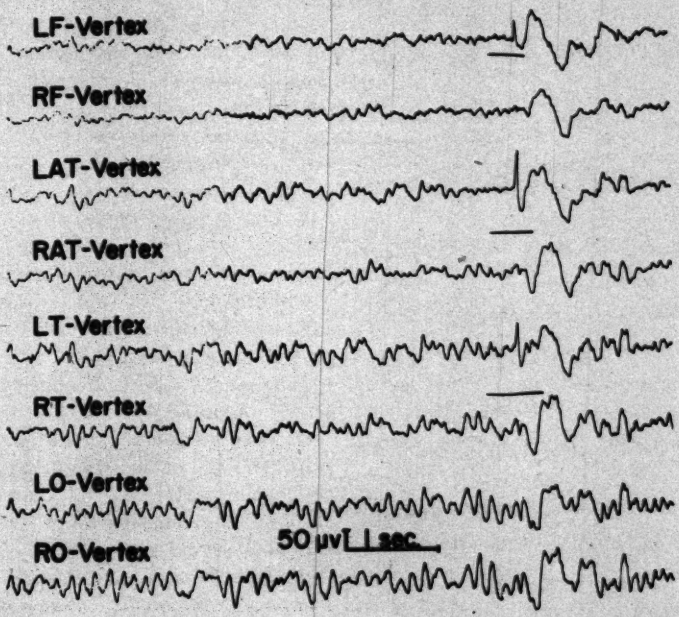

FIG. 2. Fragment of EEG record obtained from Case 2 after administration of Chloralose, 500 mg.

lucinations, delusions of persecution and grandiose ideas (he regards himself at times as the son of God) then appeared.

A routine electroencephalogram showed no definite abnormality. Activation with 500 mg. Chloralose[2] produced occasional bursts of generalized paroxysmal slow activity, but also a well defined left anterior temporal-left temporal spike focus with occasional but not consistent spread to the left frontal (Figure 2).

Case 3: Fetishism-Transvestism

The patient, a nine-year-old boy with an effeminate manner, since the age of four or five had had an almost uncontrollable urge to put on various articles of feminine attire. He was first attracted to a skirt, which he would put on and then dance about, enjoying the sight of its "flaring out." Recently his interest has included women's shoes, which are "put on the moment he sees them," and also head scarves. The patient's mother was struck by the compelling quality of his urge: "He becomes very excited; there is no ending point—he wants to go more and more—to find something more to put on—it pushes him to keep on going." The patient may keep up the dressing activity for hours, and will display no embarrassment if seen by others. He has also enjoyed playing with dolls.

Birth history was normal. There was a disturbance in sleep pattern in early infancy, and he cried "almost constantly" for the first six months of life. He displayed marked motor hyperactivity in infancy, and moderate hyperactivity was still present.

There was no history of epilepsy, but the patient had had two episodes of syncope.

[2] Ideally, electroencephalographic studies should include attempts at activation when routine recordings are normal. Drugs used in this study were Metrazol and Chloralose. Chloralose has proved to be a potent activator not only in epileptics but also in patients with behavioral disorders of impulsive type (9).

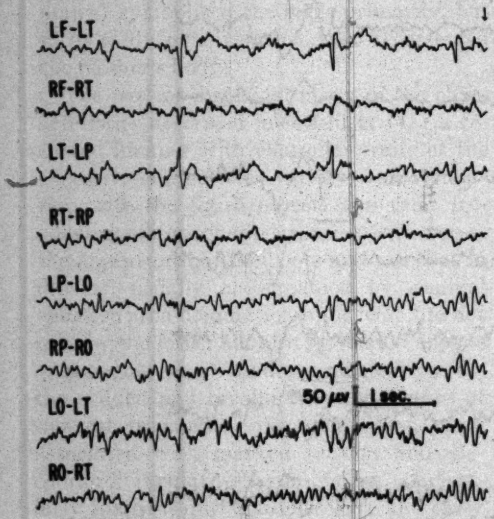

FIG. 3. EEG record of Case 3, showing left temporal spike focus.

Enuresis was present until the age of six years. Although outwardly alert and intelligent, he had had a learning difficulty, particularly in reading and arithmetic.

A routine electroencephalogram showed a well defined left temporal spike focus, with a frequent accompanying slow wave (Figure 3).

Case 4: Transvestism

The patient, a 51-year-old married man, a college graduate of high intellectual attainment, had had a compelling urge throughout his life to wear feminine attire. The patient's earliest memory of this urge was that, when he was three or four, his grandmother had placed a veil over his face and this act had filled him with pleasure. At the age of five he had had a desire to wear a raincape, and then dresses. At that age he had spent an entire afternoon putting on and taking off dresses. At the age of 28 he had begun to buy women's clothes. The need to cross-dress became increasingly intense, and had been very marked since the age of 38, so that now he does so "at every free moment."

The patient said he experienced a sense of great calm and contentment as soon as he put on women's clothes, but no desire for self-stimulation. He also noted an enhancement in his ability to perform motor acts (*e.g.*, typing) when cross-dressed. His interest was not in any specific article of attire but in the "total ensemble." He would become aroused by seeing a "beautifully dressed" woman. Not effeminate in manner, he nevertheless felt that he was not a man, and verbalized his wish to become a woman in the physical sense.

The patient had had occasional nocturnal emissions in adolescence, but had never experienced a seminal ejaculation while awake until the age of 30. Masturbation was very infrequent. Heterosexual intercourse had been almost non-existent, and there were no homosexual interests. The patient had had occasional dreams, not accompanied by seminal emissions, in which he would see well dressed women, or himself dressed in women's clothes.

There was no history of epilepsy or of childhood hyperactivity. There were very rare episodes of enuresis in early life. The patient's mother had had somnambulism.

An electroencephalogram done when the patient was 45 years of age was normal. A routine electroencephalogram done at age 51 showed low amplitude sharp waves over the left temporal electrode (Figure 4), occasionally assuming a spike configuration. Activation procedures have not been attempted in this patient.

Case 5: Transitory Fetishism-Transvestism

The patient, a boy of 12, had encephalitis following rubella at the age of five, after which he developed frequent grand mal as well as apparent petit mal and minor motor seizures, intellectual defect and behavioral symptoms (lability and unpredictability of mood, distractibility, lack of social judgment and inhibition, and occasional hyperactivity).

There was no unusual sexual behavior

until the age of 12 when, for a period of about one week, the patient displayed an "intense interest" in his sister's slips. He would put on a slip in a repetitive fashion and could not be dissuaded from this activity. The patient was also seen on several occasions to undress and look at himself in a mirror.

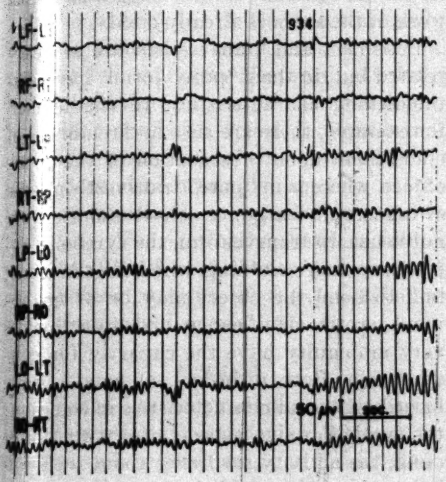

FIG. 4. Fragment of routine EEG, Case 4 (see text).

A routine electroencephalogram done when the patient was ten years of age was abnormal: the record showed almost continuous high voltage mixed delta and theta waves, as well as spike-wave complexes, in a generalized distribution (Figure 5). There was no evidence of localization to the temporal area.

With the exception of the mental status, neurologic examinations have been normal. The clinical diagnosis is that of diffuse brain damage, post-encephalitic.

DISCUSSION

Of the four patients with true fetishism or transvestism (Cases 1–4), three had definite electroencephalographic abnormalities; in the fourth (Case 4), there was a suggestive abnormality. In three of the patients these abnormalities were localized to the

FIG. 5. EEG record of Case 5.

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temporal area; the fourth (Case 2) showed a focal temporal disturbance in the presence of a generalized electroencephalographic abnormality. Only one patient (Case 2) had overt seizures. Certain clinical features were present in the others which may be viewed as possibly associated with brain dysfunction: hyperactivity (Case 1); disturbed sleep pattern in infancy, hyperactivity, enuresis, learning difficulty (Case 3); maternal somnambulism (Case 4).

In Case 5 (included in this series because of theoretical interest), transitory cross-dressing behavior occurred in an individual with evident brain dysfunction, manifested by seizures, varied behavioral symptoms and intellectual defect. The dysfunction was diffuse, with no apparent temporal localization.

It is hardly to be expected that electroencephalographic study of patients with fetishism or transvestism will uniformly reveal an abnormality, but the high frequency of such abnormalities in this series is noteworthy. Further, although the neurologic data so far accumulated in the literature and in this series do not always indicate a temporal lobe origin either of electroencephalographic abormalities or of seizures, should these be present, nevertheless the high frequency of implication of this area is striking.

The clinical manifestations of dysfunction in the temporal (more properly limbic) system are not well understood and cannot be fully catalogued—beyond commonly recognized seizure patterns. Methods of detecting such dysfunction are also limited, once one has exhausted the aid offered by the electroencephalogram. Because of the imperfectly developed state of clinical knowledge, it is not possible at this time to know whether certain disturbances in sexual behavior are in themselves, at least in part, manifestations of dysfunction in temporal and/or limbic mechanisms. There is clinical evidence, however, that the temporal lobe plays a role in the regulation of endocrine mechanisms related to sexual behavior (1). Further, an association between temporal lobe dysfunction and altered sexual behavior on the symbolic level is suggested in the work of the authors already cited.

Fetishism and transvestism are strongly related but, in their "pure" forms, have distinguishing characteristics. In the former, for example, there are ease and readiness of ejaculation upon presentation of the fetish object, whereas in "pure" transvestism, ease of orgasm and urge for self-stimulation are not usual. In transvestism, the symbolic object is women's clothing, which is then worn; in fetishism, the object may be of no obvious erotic significance and thus by its very ambiguity have the capacity for condensation. Both disorders, of course, have basic behavioral characteristics in common: the symbol or sign aspects of sex are dominant; the activity revolving about the symbol or sign often yields intense pleasure and is "imperative" and repetitive in nature; there is strong identification with the original sexual object (mother) and a need to become as one with this object.

In fetishism and transvestism, one is apparently dealing with neuropsychologic mechanisms of imitation, identification (5) and sexual arousal. These mechanisms depend for their original formation on sensory stimuli, which may later acquire sign or symbol significance. It would appear that the male (and fetishism and transvestism are primarily disorders of the male) normally tends to utilize, and may often be dependent upon, specific external stimuli for purposes of sexual arousal. It is possible that the temporal (perhaps more properly limbic) system may subserve psychic mechanisms concerned with sexual arousal and related processes of imitation and identification. Dysfunction in this neural system—whether as a result of focal disturbance or as a participant in a generalized disturbance—may prevent the proper subordina-

tion and integration of such psychic mechanisms. Sign and symbol become dominant in sexual activity and serve their primary role not only as stimulus but also as object of consummation.

It should be noted that, at least in fetishism, the disturbance is not limited to sexual activity but is more widespread, involving varied behavioral areas. It has been postulated that the basic defect in fetishism is one of "increased organismic excitability"—itself dependent upon dysfunction of the brain, particularly temporal lobe mechanisms (4). The relationship between such a postulated brain disturbance and the psychodynamic patterns of the fetishist has been previously discussed (4).

SUMMARY

Evidence has accumulated in the literature suggesting a relationship between fetishism and tranvestism, and brain (particularly temporal lobe) dysfunction.

Four new cases of fetishism and transvestism are presented. Three had electroencephalographic abnormalities with focal temporal abnormality; one, a suggestive temporal abnormality. A fifth case is presented where transitory cross-dressing occurred in an individual with generalized brain dysfunction.

The findings support the hypothesis of a relationship between these disorders and brain dysfunction. It is suggested that such dysfunction involves temporal lobe mechanisms which may, particularly in the male, normally subserve sexual arousal patterns.

REFERENCES

1. Anastasopoulos, G., Diakoyiannis, A. and Routsonis, K. Three cases of temporal lobe epilepsy with endocrinopathy. J. Neuropsychiat., **1**: 65–76, 1959.
2. Clark, L. P. The nature and pathogenesis of epilepsy. New York J. Med., **101**: 385–392, 442–448, 515–522, 567–573, 623–628, 1915.
3. Davies, B. M. and Morgenstern, F. S. A case of cysticercosis, temporal lobe epilepsy, and transvestism. J. Neurol. Neurosurg. Psychiat., **23**: 247–249, 1960.
4. Epstein, A. W. Fetishism: A study of its psychopathology with particular reference to a proposed disorder in brain mechanisms as an etiological factor. J. Nerv. Ment. Dis., **130**: 107–119, 1960.
5. Hutchinson, G. E. A speculative consideration of certain possible forms of sexual selection in man. Amer. Naturalist, **93**: 81–91, 1959.
6. von Krafft-Ebing, R. *Psychopathia Sexualis*. Physicians & Surgeons Book Co., New York, 1931.
7. Lukianowicz, N. Survey of various aspects of transvestism in the light of our present knowledge. J. Nerv. Ment. Dis., **128**: 36–64, 1959.
8. Mitchell, W., Falconer, M. A. and Hill, D. Epilepsy with fetishism relieved by temporal lobectomy. Lancet, **2**: 626–630, 1954.
9. Monroe, R. R., Jacobson, G. and Ervin, F. Activation of psychosis by a combination of scopolamine and alpha-chloralose. A.M.A. Arch. Neurol. Psychiat., **76**: 536–548, 1956.
10. Petritzer, B. K. and Foster, J. A case study of a male transvestite with epilepsy and juvenile diabetes. J. Nerv. Ment. Dis., **121**: 557–563, 1955.
11. Yawger, N. S. Transvestism and other cross-sex manifestations. J. Nerv. Ment. Dis., **92**: 41–48, 1940.

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A NEW APPROACH TO THE PSYCHOLOGY OF CRIME[1]

EDMUND BERGLER, M.D.[2]

Three problems are paramount in psychiatric criminology:

1) Is the criminotic act a volcanic eruption of repressed aggression?

2) Is the inner guilt, frequently leading to self-betrayal and legal punishment, connected with that aggression, and what, if any, is the structure of the inner conscience (superego) of the criminotic?

3) In addition to unknown biologic factors, are social and environmental influences decisive?

Most of the scientific literature states that the theory of volcanic eruption of aggression is valid, that the criminotic has not developed a workable inner conscience, and that biologic and environmental factors are paramount. For example, Zilboorg (13) said: "The criminal commits the criminal act when his instinctual drives temporarily overcome the resistance of the superego and thus overwhelm the ego, which is forced to do the bidding of the id." Alexander and Healy (1) conclude: "Thus, one cannot discard an earlier assumption of Alexander that certain unacquired bases of the instinctual life (constitution), apart from environmental influences, must be partly responsible for the fact that similar emotional conflicts may, depending on the make-up of the individual, result either in criminality or in neurosis." Although the difference between neurosis and criminosis (the term created by A. N. Foxe) is theoretically acknowledged, both clinical entities are frequently put in the same category, a misconception that prompted Karpman (10) to issue the warning: "The psychoanalytic approach has as yet failed to contribute significantly to the solution of the problem because it gratuitously went on the assumption that the same mechanisms operated in criminals as in neurotics... The great majority of the professional and habitual criminals must be approached by a method different from that used for neurotics." Otherwise, the literature excels in either circumlocution or vagueness. Sheldon and Eleanor Glueck speak about "numerous factors, various circumstances, and uncontrollable forces either of psychological or social origin," as well as the biologic factors responsible for the facts of crime (9). Finally, some authors shrug their shoulders and declare flatly: "There is no scientific knowledge in the field of criminology. We have no knowledge of the causes of criminal behavior" (11).

The first—though groping—breakthrough in that vast area of "We don't know" came when the idea presented itself that the criminotic act is not a simple eruption of unconscious forces but a complex chain of unconscious defense mechanisms. Nearly simultaneously, Schilder and Keiser (12) and Alexander and Healy (1) presented that concept, though in a tentative fashion. Especially significant were the observations by Schilder and Keiser of murderers committed to the Bellevue Psychiatric Hospital for examination. The authors concluded that "the aggressive action is a reactive state resulting from a sense of passivity." That passivity was equated by the authors with femininity.

In writing of case material comprised mainly of crimes of theft, Alexander and Healy allude to pseudo-masculinity and orality, but seem to understand under those terms only the original wish to *get*, not the later masochistic elaborations.

The merits of the formulations of these four authors overbalance all objections to

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ther , though some of these objections are weighty—*e.g.*, Schilder and Keiser identify passivity and homosexuality, although homosexuality has a different basis (4). Alexander and Healy's material comprises selective cases, and they take the wish to *get* as a direct remnant of the baby's "gimme" phase, overlooking the fact that, in adult neurotics, this wish has become an inner defense covering the masochistic elaboration leading to the wish to be refused (2, 5).

Even after the first hurdle was surmounted and the idea of inner defenses introduced, the basic question remained: Why should not these inner defenses be expressed in neurotic symptoms and signs, instead of requiring the criminotic outlet? The answers are disappointing. Schilder and Keiser state: "One would conclude that aggressiveness comes to criminal acts when we deal either with specific defects in the ego structure or with an overpowering of the ego system by particular libidinous conflicts of special strength or primitiveness." Alexander and Healy speak of the tensions which "seek a realistic expression in criminal acts and cannot be relieved by mere fantasy products that are exhibited in neurotic symptoms."

Superimposed is the fallacy, encountered in many studies by other authors, that unconscious motives decipherable in neurotics are identical with those in criminotics. Thus, discovery of an Oedipus conflict in a criminotic seems to satisfy these researchers. Freud himself objected to this naïve assumption, alluding to the *cause célèbre* of Halsman in pre-Hitler Austria. The reactionary medical faculty of the University of Innsbruck was asked by the Court for its opinion of the reasons allegedly prompting a young Latvian Jew to kill his father, a crime which the defendant denied and for which there was only the flimsiest circumstantial evidence. The Nazi-infested faculty decided that Halsman had an Oedipus complex that was "operative." Freud objected to this biased nonsense, pointing out the universality of the Oedipus complex. Freud said: "Even if the conflict between father and son could be proven, one must say that there is a great distance between this conflict and the causative factors of such a crime." Freud illustrated his point with a joke: A jury finds a man guilty, as charged, of robbery, convinced by the circumstantial evidence adduced at his trial: he had been seen in the vicinity of the robbed apartment and, when arrested, had been carrying a skeleton key in his pocket. When the judge asked the defendant if he had anything to say before sentence was passed, the defendant replied that he wished to be punished for adultery, too, since he also had that "key" in his pocket (8). The "great distance" stressed by Freud is exactly the distance between naïve ideas on the causation of crime and the "specific factor" actually leading to crime.

As the problem stands, the only way to avoid hopeless confusion is to make a precise differentiation between *two* factors in every criminal act: *the variable* and *the constant*. In numerous publications, starting in 1943 (7), I have suggested this formulation: The *variable factor* is made up of the psychologic content, and is multitudinous in form, differing in individual cases. The variety of motives for a crime is as great as the variety of unconscious motives in general. The *constant factor* in crime is the X which explains the motor act executing the criminal move itself. This constant and pathognomic factor I have proposed calling the "mechanism of criminosis."

The differential diagnosis between these two factors must take into account these facts: The *variable* factor explains the unconscious contents of a criminal action. To explain those, we must use all the knowledge of unconscious mechanisms discovered by Freud and successfully applied to the explanation of human conduct in general: elaboration of unconscious wishes in staggering defense mechanisms, projection, identification, atonement of unconscious guilt

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feelings, self-damage, and so on. The *constant* factor, the mechanism of criminosis, refers not to the variable psychologic contents of a specific crime, but to the motor act executing the results of the variable factor. It borrows from the seemingly inexhaustible source of inner aggression, without showing on the surface whether real or defensive pseudo-aggression is at stake.

In searching for the mysterious "mechanism of criminosis," one is struck by an apparent similarity which is encountered in all neurotics—the *mechanism of psychic masochism*—and which goes back to unresolved conflicts with the earliest image of the mother (the pre-Oedipal mother). These neurotics enact a drama consisting of three parts, a drama I have described and elaborated upon (2):

1) Unconsciously, they construct (or misuse) a situation in which someone is refusing them, or is denying them, or is being cruel to them.

2) Not aware that they themselves have started the ball rolling, they misread the situation and act as though the misfortune had started with the provoked person's inexplicable malice (actually, retaliation); against the latter they invoke the right of seeming self-defense, and fight with righteous indignation ("pseudo-aggression").

3) Having unconsciously chosen an adversary too strong to defeat and a situation in which they cannot win (winning was never intended, unconsciously), they wallow in self-pity, retire to the lachrymal corner and bewail their fate: "This can happen only to poor little me."

The criminotic seems to follow the same pattern. (The misuse of social factors is typical, though no reasonable person denies the importance of social factors, *per se*.) Still, there is *something else involved*. To the neurotic, it seems, the neurotic pattern suffices as proof that he is *not* passively helpless; moreover, the inner wish to suffer seems satisfied in the disadvantages incurred by the provocative action. For example, if a neurotic husband constantly provokes his wife, the scenes she makes in retaliation are obviously a far cry from legal retaliation, in the form of the electric chair, if the husband actually killed his wife in a quarrel.

What it amounts to, practically, is *the ratio between masochistic helplessness and pseudo-aggressive defenses needed* in these two cases. Both—the neurotic and the criminotic—fight with inner helplessness and masochistic passivity; the alibis and defenses are different. The motor act in criminal actions does not prove that the criminotic is more aggressive. Exactly the opposite is true. *The criminotic is the most passive-masochistic person in the world; he requires as inner alibi an over-inflated herostratic act.*

The situation is comparable to that of a dwarf who tries to annoy a giant who refuses to see these attempts. To prove that the alleged avenger is not helpless at all, he uses—so to speak—dynamite. Of this the giant *must* take cognizance. True, the dynamite will blow both giant and dwarf to pieces, but the helplessness is overcome. (At what a price!)

The feeling of helplessness has nothing to do with "feeling of inferiority," feeling of being unmanly and feminine, "prestige" and so on. It is a sensation which the infant has *before* these other terms have any meaning for him. It is this deep feeling of dwarfism which the child in the criminal cannot endure. Secondarily, masculine and feminine identification and defense against passivity take place, as Schilder and Keiser have shown; these, however, represent a later reflection of an early development.

Even in neurotic psychic masochism, the aggression displayed is spurious; it is pseudo-aggression, covering deep masochistic passivity. That spurious show of aggression is of the alibi type, and leads directly to the problem of alleged absence of a workable inner conscience (superego) in criminotics.

That the criminotic is simply without "conscience" seems absurd to me (6). It means that the observers have been misled by the unfamiliar technique used by the criminotic's conscience. Under typical, not-too-neurotic circumstances, the inner conscience prevents asocial acts to a limited degree. The criminotic's conscience works differently: it gives the victim "enough rope to hang himself." In other words, *punishment is unconsciously calculated in the criminal act.* To judge by the results—the electric chair, or a long prison term—the criminotic's conscience is even more severe than the average conscience, and the penalties inflicted are sterner.

The superego makes itself clinically visible in its effects: feeling of guilt, need for punishment, sense of depression. All of the "little mistakes" which criminals make and which rule out "a perfect crime" form a pattern which has its affective basis exactly in the allegedly absent conscience of the criminotic.

The next problem is to what degree the feeling of guilt is the simple consequence of a criminal deed. How complicated this problem is becomes apparent in comparing the following statements:

Schilder and Keiser: "We have no definite reason to believe that the wish for punishment, although present, is one of the outstanding factors . . ." (12, p. 368). "As with most criminals, he wants to be punished, but not too severely. It is the attitude of the child, who regains the love of his parent after punishment" (12, p. 367).

Zilboorg: ". . . As soon as the impulse is discharged and the special Id drives are thus temporarily gratified and silenced, the Super Ego reestablishes itself and asserts its demands. Even the hard, defiant criminal then feels unconsciously repentant. His challenging, snarling, boisterous defiance of the law, or his sullen, apparently indifferent, emotionless attitude is in most cases but an automatic covering, boastful or humbled, of the sense of guilt. The writer has never failed to find it deeply buried in the unconscious of apparently confirmed criminals of whom he had the opportunity to make a psychological study within the walls of a prison. Many criminals, as a result of this inner penance, kill themselves soon after the crime . . ." (13).

I disagree with all three authors as to the deposition of the feeling of guilt. I believe that the hidden or overt guilt pertains to the masochistic basis, *secondarily* shifted to the pseudo-aggressive action. This represents the mechanism of "taking the blame for the lesser intrapsychic crime" (6). I disagree with Schilder and Keiser in that I believe that the *unconscious feeling of guilt has the place of pivotal importance in criminal deeds and is automatically included in the deeds.* If the criminal did not know unconsciously that he would be punished, if he did not project his expected punishment upon the juridical and penal authorities, making them the executive organ of his own superego, his feeling of guilt would prevent his deed in the first place. Only because he unconsciously projects this expectation of punishment does he appear often detached and sometimes without penitence.[3]

I disagree with Zilboorg because of his conception of crime. For him, the criminal action represents a volcanic eruption of repressed id-wishes, followed by a feeling of guilt. In my opinion, crime is not the outburst of an id-wish but a defense against it, executed by highly complicated means. The feeling of guilt does not appear *ex post facto,* but is embedded in the deed itself. *Only this unconscious expectation of punishment makes the criminal action possible.*

Moreover, I believe that most criminotics are schizoid or schizophrenic personalities.

[3] Interestingly enough, Freud suspected long ago that some criminal actions were performed because the criminal harbored an unconscious guilt stemming from other sources. He spoke of "Verbrecher aus Schuldgefuehl." In these cases the feeling of guilt was not the result of the criminal deed, but the criminal deed the result of a feeling of guilt.

Recently I have suggested the thesis (3) that, for these very sick personalities, the pseudo-aggression included in the psychic masochistic manifestations is in itself a defense against the unconscious fantasy of being—in complete helplessness—manipulated by the image of the mother. Hence our poor therapeutic results with these people.

We have no "masochistometer," and therefore we cannot judge the quantity and quality of psychic masochism in criminotics. We can only suspect that the quantity is increased in criminotics, and speculate on whether the eighteenth century philosopher, Hegel, was correct when he claimed that there is a point at which quantity changes into quality.

One fact is clear: no punishment prevents crime. Responsible for this fact is *the unconscious calculation on punishment in the criminal's deed.* It is an integral part of the crime. The dread of punishment can therefore not forestall crime. Paradoxically, to quote an ironical patient of mine, a criminal lawyer, dread of punishment is more necessary for non-criminals than for criminals. The non-criminal needs unconscious recompense for being a lifelong good boy. Therefore, public opinion asks for severe punishment of criminals. The majority of habitual criminals are unchangeable. This is only a reminder to be less optimistic about the penal results, especially in the case of second or third offenders, and to have fewer illusions about persons having "learned their lesson." People in general—and criminotics in particular—do not learn so easily.

Every society must protect itself against individuals who do not accept the rule of the community. Criminal deeds are outside the social game; they represent specific solutions of a specific inner conflict, and endanger the community as a whole. The habitual criminal himself has nothing to hope for from the solution of the enigma of crime. He will very likely be treated *morally* differently; he will be acknowledged to be a sick person. *De facto,* he will pay for the omission of moral odium with disadvantages—for instance, lifelong detention. The moment the illusion of betterment and rehabilitation of habitual criminals through punishment is abandoned because it does not work, society will regretfully impose lifelong detention on the habitual criminal, not because he is "bad," but because he is a danger to the community. Whether a future therapy will be capable of helping him is today a matter of conjecture.

Our great hope for the future is that criminal psychopathology will, after overcoming its childhood difficulties, become a real help—perhaps even a therapy, although I doubt it. Perhaps the solution of the criminologic problem will lie in child analysis, when we no longer wait until the damage is done, but treat difficult children in a preventive way. At present, criminal psychopathology is still in the stage of sporadic experimentation, constant hope, abundant theories—and a very real lack of clinical experience.

REFERENCES

1. Alexander, F. and Healy, W. *Roots of Crime,* p. 289. Knopf, New York, 1935.
2. Bergler, E. *The Basic Neurosis.* Grune and Stratton, New York, 1949.
3. Bergler, E. *Curable and Incurable Neurotics.* Liveright, New York, 1961.
4. Bergler, E. *One Thousand Homosexuals.* Cooper Square Publishers, New York, 1959.
5. Bergler, E. *Principles of Self-Damage.* Philosophical Library, New York, 1959.
6. Bergler, E. *The Superego.* Grune and Stratton, New York, 1952.
7. Bergler, E. Suppositions about the mechanism of criminosis. J. Crim. Psychopath., **5:** 215–246, 1943.
8. Freud, S. *Gesammelte Schriften,* Vol. 12, p. 412. Int. Psychoanal. Verlag, Vienna, 1930.
9. Glueck, S. and Glueck, E. *Criminal Careers.* Knopf, New York, 1940.
10. Karpman, B. *The Individual Criminal,* p. vii. Nervous and Mental Disease Publishing Co., Washington, D. C., 1935.
11. Michael, J. and Adler, M. J. *Crime, Law, Social Science.* Harcourt, Brace, New York, 1933.
12. Schilder, P. and Keiser, S. *A study of criminal aggressiveness.* Genet. Psychol. Monogr., **18:** Nos. 5 and 6, 1936.
13. Zilboorg, G. *Mind, Medicine and Man,* pp. 253–254. Harcourt, Brace, New York, 1947.

CRYOSTATIC CONGELATION: A SYSTEM FOR PRODUCING A LIMITED, CONTROLLED REGION OF COOLING OR FREEZING OF BIOLOGIC TISSUES[1]

IRVING S. COOPER, M.D.[2] AND ARNOLD ST. J. LEE[3]

This paper reports a new technical method of clinicophysiologic neurosurgery in human beings. The method is based on the fact that limited cold blocks nervous conduction, whereas excessive cold can produce irreversible congelation and ultimate necrosis of nervous tissue.

In basal ganglion surgery for the relief of involuntary movement disorders, it has been our basic premise that one must produce a reversible lesion of temporary blockage of nervous conduction in any particular site deep within the brain in a conscious, co-operative patient before one can safely proceed with the infliction of a permanently destructive lesion (1). In our earlier investigations we injected procaine into selected sites within the basal ganglia before infliction of a permanent lesion. During the last several years a small balloon has been inflated within the intracerebral target site to produce this temporary effect upon a localized region of the brain. This principle should be followed in any neurosurgical procedure aimed at producing such functional results as relief of pain or spasticity.

Obviously a clinical or physiologic test should be carried out before destruction of any type of nervous tissue, either central or peripheral. The necessity for doing so in order to achieve relatively certain and safe results from deep intracerebral surgery is well documented. Consequently, a search for improved methods of production of localized neural blockage and lesion creation has paralleled our studies in functional neurosurgery.

A special cannula for use in intracerebral or peripheral surgery in human subjects has been developed which can produce both physiologic blockage of nerve conduction and ultimate neural destruction. Douglas and Malcolm (4) and many others have demonstrated the usefulness of cold in producing temporary cessation of nervous conduction in peripheral nerves of animals. Gaillard (5) has reported the effects of local refrigeration on peripheral nerve in man, and Denny-Brown and his associates (3) have described the temporary and permanent effects produced by cold in human nerves. Rowbotham and his associates (6) applied freezing temperatures to intracerebral tumors in three patients. They employed a cooling cannula in which a mixture of carbon dioxide and acetone was circulated. The cannula, which was not insulated, produced a solid core of frozen brain tissue throughout the segment that was inserted into the brain and tumor. No untoward effects were noted.

During our investigations an instrument has been developed which may serve as a brain cannula for deep intracerebral surgery, or to deliver cooling and freezing temperatures to peripheral nerves. The mechanical aspects of the cannula and its spatula attachment for nerve cooling are shown in Figure 1; the actual appearance of the cannula, in Figure 2.

The source of cooling used in this system is liquid nitrogen, with a temperature of minus 196°C. The nitrogen enters the brain cannula through a flexible insulated coil under a pressure of 22 pounds per square inch above atmospheric pressure. The liquid nitrogen passes through an inner tube to

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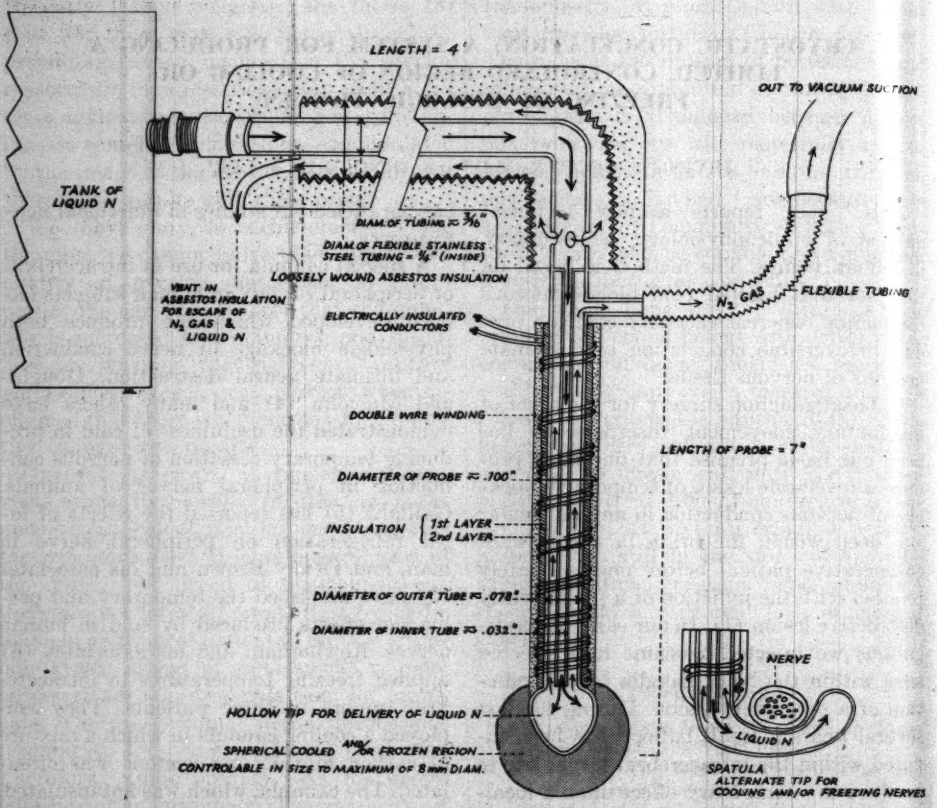
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DOUBLE WIRE WINDING
LENGTH OF PROBE = 7"
DIAMETER OF PROBE = .700"
INSULATION
1st LAYER
2nd LAYER
DIAMETER OF OUTER TUBE = .078"
DIAMETER OF INNER TUBE = .032"
NERVE
HOLLOW TIP FOR DELIVERY OF LIQUID N
LIQUID N
SPHERICAL COOLED AND/OR FROZEN REGION CONTROLABLE IN SIZE TO MAXIMUM OF 8 mm DIAM.
SPATULA ALTERNATE TIP FOR COOLING AND/OR FREEZING NERVES

FIG. 1. Diagrammatic illustration of mechanism of cooling cannula.

reach the tip of the cannula, the freezing effect is dissipated at the tip, and the resultant gaseous nitrogen escapes through an outer tube and is removed through tubing attached to a vacuum pump producing a pressure of 15 pounds per square inch below atmospheric pressure.

A particularly significant aspect of this cannula is the fact that, except for its tip, the entire length of the cannula is insulated by heat from an electrically conductive copper wire with a large temperature coefficient of resistance (Figures 3 and 4).

The cannula itself (that is, both the inner and the outer tubes) is constructed of non-corrosive, high strength, hard stainless steel. The length of the cannula, because of its heated insulation, is maintained at body temperature, and the cooling or freezing effect is limited to the non-insulated tip. In the cannula constructed for use in basal ganglion surgery (cryothalamectomy) (2), freezing takes place up to a maximum of 4 mm. on each side of the center of the tip, thus producing a lesion with a maximum diameter of 8 mm.

In vitro experiments carried out in gelatin with a specific gravity approximating that of the human brain, at room temperature, demonstrate the consistency and reliability of lesion formation at the tip of this cannula. Animal experimentation dem-

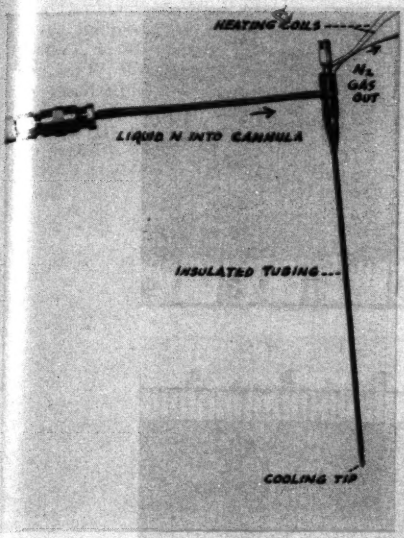

FIG. 2. Photograph of cooling cannula.

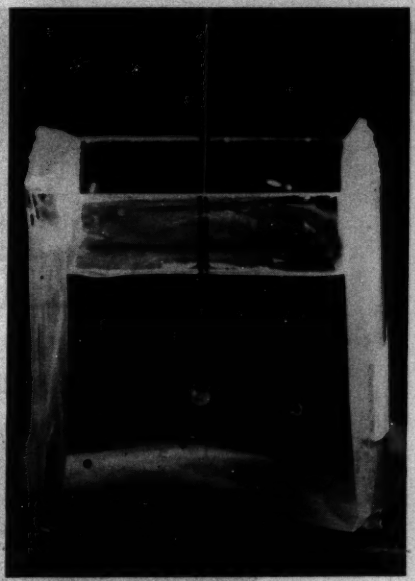

FIG. 4. Spherical frozen lesion produced about cooling tip of cannula during two minutes of passage of liquid nitrogen through the tip. Heated insulation of remainder of cannula prevents any spread of freezing effect.

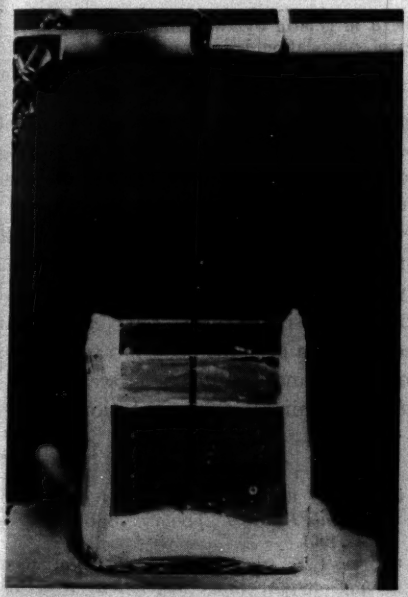

FIG. 3. Cannula placed in gelatin bath to demonstrate lesion formation in gelatin surrounding the cooling tip of the cannula.

onstrates solid freezing of a spherical ball of cerebral tissue up to 8 mm. in diameter, exactly analogous to that so clearly visible in the gelatin experiment (Figure 5). The lesion is fully formed within two minutes when liquid nitrogen is used as the cooling agent. Other refrigerant solutions can be employed in the same system if one wishes to conduct physiologic experiments in a different temperature range.

Certain advantages are immediately apparent in the use of an insulated system such as that described in this report for producing controlled cooling of a limited region of biologic tissue. In the case of deep intracerebral surgery, such as lesion production within the thalamus for relief of involuntary movement disorders, the possibilities of producing temporary physiologic testing and permanent lesion infliction by the same rapid means are obvious.

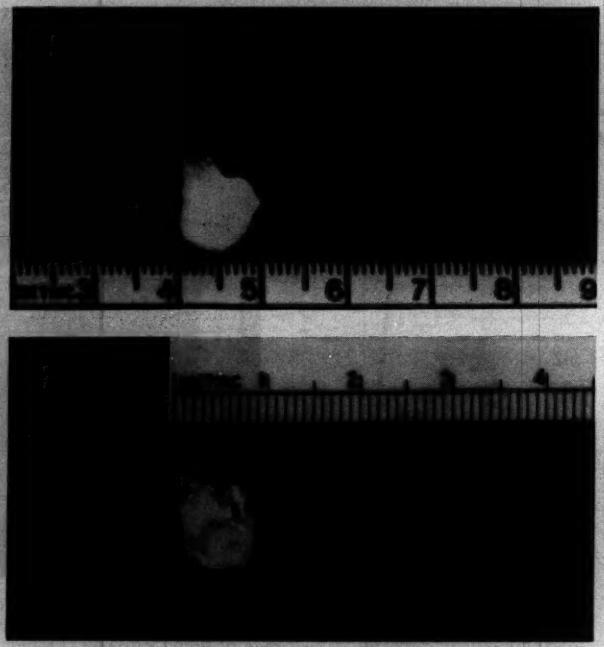

FIG. 5. A: Close-up of congelation lesion produced in gelatin by two minutes of liquid nitrogen freezing through hollow tip. B: Close-up of congelation lesion produced in cat's brain by same method.

Moreover, our experience thus far indicates that this method of lesion production does not carry the risk of producing intracerebral bleeding, so that the risk of mortality should be virtually nil.

The spatula may be employed for temporary or permanent destruction of cranial and peripheral nerves. In the case of tic douloureux, this system could produce physiologic blockage of nervous conduction as a clinical test, and a more lasting but not permanent involvement of the nerves for clinical relief of pain. Its use in certain peripheral nerves to abolish spasticity in selected cases of paraplegia is also being investigated.

SUMMARY

An instrument is described which can produce a limited, controlled region of cooling or freezing of biologic tissue. It can be used to provide temporary physiologic blockage of nerve conduction in a limited area of neural tissue within the central or peripheral nervous system. It can also safely produce, deep within the brain (*e.g.*, cryothalamectomy), a spherical, limited lesion of consistent size. This cannula has been used to produce deep intracerebral lesions in human subjects as well as in *in vitro* and animal experimentation.

REFERENCES

1. COOPER, I. S. *Parkinsonism: Its Medical and Surgical Therapy*. Thomas, Springfield, Illinois, 1961.
2. COOPER, I. S. AND LEE, A. ST. J. Cryothalamectomy—hypothermic congelation: A technical advance in basal ganglia surgery. J. Amer. Geriat. Soc., **9**: 714–718, 1961.
3. DENNY-BROWN, D., ADAMS, R. D., BRENNER, C.

AND DOHERTY, M. M. Pathology of injury to nerve induced by cold. J. Neuropath. Exp. Neurol., **4**: 305–323, 1945.

4. DOUGLAS, W. W. AND MALCOLM, J. L. The effects of localized cooling on conduction in cat nerves. J. Physiol., **130**: 63–71, 1955.

5. GALLARD, R. Effets de la réfrigération locale sur les processus d'excitation des nerfs moteurs in situ chez l'homme. C. R. Soc. Biol., **149**: 2090–2092, 1955.

6. ROWBOTHAM, G. F., HAIGH, A. L. AND LESLIE, W. G. Cooling cannula for use in the treatment of cerebral neoplasm. Lancet, **1**: 12–15, 1959.

SARASON, SEYMOUR B., DAVIDSON, KENNETH S., LIGHTHALL, FREDERICK F., WAITE, RICHARD R. AND RUEBUSH, BRITTON K. *Anxiety in Elementary School Children.* Wiley, New York, 1960. viii + 351 pp. $7.75

The senior author has a solidly established reputation in the fields of personality dynamics and intellectual retardation; his *Psychological Problems of Mental Deficiency*, first published by Harper in 1949, has gone into three well accepted editions, and he has since produced other major work. About ten years ago he became actively interested in the relationship of anxiety to learning and, with the collaboration of a number of junior colleagues, began to publish the results of a series of investigations in which college students served as subjects. In these studies no physiologic indices were used; behavioral measures, based on self-reports of general anxiety level, were typically used to distinguish "high" and "low" anxiety subjects, who were stressed and observed in terms of some such dependent variable as test performance. The present volume is, in effect, an extension and elaboration of this earlier work. In it is reported a six-year program of research on anxiety in children of elementary school age. Much of the material appears for the first time here, although a certain amount has been published in journal articles. The co-authors, all associated with the Department of Psychology at Yale, were members of the research team.

There is merit in presenting a block of research studies in book form. It allows of more opportunity for discussion and theoretical speculation than does the leanly compressed periodical report. It makes possible a wider frame of reference, in which the relationship of one investigation to another can be seen. It gives scope and space for meaningful data and ancillary findings that must be culled from the brief presentation. It provides a field for broad generalizations with multiple bases of support. And so on.

There are disadvantages and pitfalls as well. An organized compilation of small studies almost inevitably is directed to a wider audience—as is this book—than were the original reports: this can make for reduced impact and lack of focus. And greater freedom for discussion can blur the data with superficial speculation.

By and large, this book avoids these dangers. It is written for the child psychologist, the psychiatrist and others in the behavioral sciences as a sophisticated, cohesive and competently reported account of a series of field studies of children. Though not so directed, it should also be of interest to a variety of people in education—administrators, guidance personnel, school psychologists—for the findings presented here obviously have potent implications for psychologic procedures and practices of the schools. To some degree the book is uneven: the first six chapters, especially the section dealing with the validation studies of the anxiety scales, are more readable and seem on surer footing than the later chapters. But none of it is dull or over-written, as are many reports of this type. A basic concept herein developed is that of "test anxiety," and a concern with factors contributing to discrepancies between intellectual potential and performance in children is an important theme of the monograph.

The introductory chapters develop the concept of test anxiety within a theoretical framework essentially Freudian, and elaborate a number of hypotheses dealing with anxiety as it modifies learning and problem-solving. There is a 50-page critical review of the literature having to do with fear and anxiety, variously defined, and a multitude of experimental and behavioral variables;

this chapter will be valued highly by research workers in the field. There follow chapters that present discussions of methodological problems rising out of variations in test-taking attitudes, descriptions of the measuring instruments, and summaries, based on data collected in both individual and group settings, of validity studies. Consideration is given at some length to environmental variables (*e.g.*, social status, parental attitudes), and to personality correlates and sex differences. A final speculative section goes well beyond the substrate of research findings to develop an array of implications of the research experience for contemporary problems in psychology and education.

The concept of anxiety, however defined (and maligned), continues to hold center stage in most psychologic theories. As applied in these studies, it is sharply delimited and operationally defined, with a resultant utility that likely will contribute to the final clarification of the concept.

The authors seem to have focused their interests on the handicapping aspects of anxiety. (They were little concerned, for example, with the facilitating action of anxiety on productivity.) Some of the findings will be put to immediate use in research investigations, and with clear profit could be applied to educational practice in the large; other results will stimulate further work in the course of time.

In the competitive atmosphere of the typical classroom, test anxiety may be observed even among some elementary school children. The reported level of anxiety increases with age, and under some conditions the problem-solving ability of "high-anxious" youngsters is significantly lower than that of children of low test anxiety. Sex differences are significant: girls report more anxiety than do boys. In terms of both cultural standards of child-rearing and the exquisitely test-conscious culture of schools in this country, this distinction is to be expected; more important, however, are the broad implications of this kind of difference for intellectual measures generally. Even in a highly controlled learning situation a child's performance may be well below his potential, his optimal ability to perform; and the analysis of factors relating to this discrepancy has immediate social meaning.

H. A. Robinson

RABIN, ALBERT I. AND HAWORTH, MARY R., EDS. *Projective Techniques with Children.* Grune and Stratton, New York, 1960. xiii + 392 pp., illus. $11.75

For the reader interested in the projective techniques currently used in clinical or research settings with children, Rabin and Haworth have performed a service: in one volume they have compiled a description of all such techniques, ranging from the Rorschach through the Blacky to CAT to MAPS, as well as drawing, play, puppetry and films. Most of the chapters include a brief theoretical rationale for the tests and a summary of research findings. In every case an extensive bibliography completes the chapter. The Rorschach receives unusual treatment in that two chapters are devoted to it; one describes its use with children, the other, with adolescents.

As is to be expected from a multi-authored work, there is variation in treatment of subject matter, in scope, specificity and caliber. One author, for example, goes into such detail as the type of carrying case available for test materials; another settles the matter of scoring by referring the reader to an appropriate reference. Uniformity of aim may not have been sought—certainly it was not achieved. Some techniques are fully described (an actual protocol is scored and summarized); others receive general treatment, apparently intended to provide only the basic rationale and a general description of the materials and procedures.

Despite this variation in treatment and scope, a generally high level of achieve-

ment is maintained throughout the book. Florence Halpern's chapter on the Rorschach is outstanding: in a straightforward fashion she relates the developmental features of the child with the type of perceptual behavior tapped by the test. (It might be well to emphasize, however, that this value judgment is being made about the *presentation* rather than the *utility* of the technique.)

An anthology has the strength of providing a discussion of each topic by an expert, but the weakness of unevenness of style, treatment, and flow of material. The editors made an effort toward unity by including an introductory chapter on projective techniques, and three chapters at the end, dealing with projective techniques in the clinic and in research, and on new developments in the field. With the possible exception of the introductory chapter, these add little to the other chapters.

A book may fulfill many functions, and this one clearly provides a compilation of a group of psychologic techniques; but it goes no further than that, either in providing a critical evaluation of the techniques, or in attempting to answer any of the cogent questions that have been bedeviling projective tests for some decades. Several of the writers are aware of the problems of reliability and validity, but in these chapters they do not go beyond the statement, made so often, that perhaps these concepts cannot be carried over unmodified from intelligence tests into projective tests. While this view may be entirely reasonable, the contribution of the book would have been enhanced had the discussion been carried forward with proposals for substitute criteria. In short, although this book does not advance projective techniques, it does provide a useful summary of the available data. Consequently, it might serve as a good text for an introductory class on projective techniques. It certainly constitutes a valuable source of references, and for some of the tests might even serve as a manual.

Sonia F. Osler

RICKERS-OVSIANKINA, MARIA A. *Rorschach Psychology*. Wiley, New York, 1960. xvi + 483 pp., illus. $8.50

The stated aim of this volume is the enhancement of a mutual awareness between "theory minded readers from the ranks of both clinical as well as more general psychologists." It consists of 14 articles on a variety of "Rorschach issues," each constituting a major research interest for its individual author, with chapters on Categories of Analysis (Location, Determinants and Content), The Test Pattern, and Problems of Scientific Appraisal. The author hopes to make the clinical users of the Rorschach more sensitive to "the place of their method within the theoretical framework of psychology at large and within the psychology of personality in particular," and to alert the general psychologist to "what a mine of wealth for their own domain can be found within the diversified pattern of a Rorschach protocol."

That the stated aims of the book are noble ones is scarcely arguable; that these aims have been achieved is perhaps questionable. The chapters often stray far afield, one, for example, being a rather interesting but fragmentary clinical report showing how one Rorschach interpreter can wring something from the most meager data. But such a display of prowess will not captivate the skeptical general psychologist, since what is squeezed out of the protocol could obviously have been inferred from the case history material. Nor will the agnostic be converted by such gratuitous, far-out interpretations as: "WM+—an acceptance of the awareness of death as a part of the totality of life." Another chapter is a scholarly enough but routine résumé of the literature on the possible meanings of one of the major determinants: form perception. It might be useful to the beginning Ror-

schach student (to whom the book is *not* addressed) but the material is rather old-hat to the experienced interpreter. The data are presented with implications no more specific than those in a standard Rorschach textbook. Indeed, throughout the collection the possible relationships between the Rorschach and general personality systems are considered only sketchily at best. The book also falls short of the mark in its attempt to "illustrate ways in which the Rorschach method can be used to provide answers to problems in basic psychological science," though it does make liberal use of plausible sounding statements pointing to seeming parallels—for example, between Werner's development theory and Rorschach data from certain groups. A chapter by the Murphys makes a rather general attempt to demonstrate the theoretical usefulness of the Rorschach for general personality theory.

As is often the case in such collections, the quality of the chapters is uneven. There are several excellent contributions, among them chapters by Fonda on white-space responses, and by Bohm on Binder's chiaroscuro system (though the latter makes no attempt to translate some of his characterologic terms, *e.g.*, "sophropsyche," into those of standard usage. Also noteworthy is Piotrowski's chapter on the movement score, in which empirical research is cited; but the evidence would have been more convincing for the general theoretician if some indication of statistical significance had been included. Singer's chapter on experience-type is a conscientious and succinct effort to adhere to the basic aims of the book, though some readers might take a dim view of his accepting Rorschach's hypotheses as valid and then speculating *why*. Suitable primarily as a possible research tool is Holt and Havel's detailed content scoring system for assessing the amount of primary and secondary thought processes revealed in a protocol. An outstanding chapter is David Shapiro's subtle but solid discussion of the relationship of perception to color response. Though perhaps most enriching to the Rorschach interpreter, it casts some valuable sidelights for the psychotherapist and personality theorist.

Two issues always of interest to the theoretical-minded are treated in the section on Problems of Scientific Appraisal, but both, unfortunately, are somewhat disappointing. A rather routine chapter on reliability points out the well known fact that there are many ways of defining it in operation, and stresses the importance of choosing the appropriate one to apply to the Rorschach. The chapter on validity is a reiteration of the cry for more research. Some of the suggestions it offers (such as the need for greater intra-individual representative sampling) are quite ill conceived and inappropriate.

Despite its attempts to place the Rorschach method within the general framework of psychologic theory, much of the time the book sounds like Rorschach expert talking to Rorschach expert: quoting the Founder; obsessing over scoring minutiae; and discussing personality within the confines of Rorschach terminology, with only sporadic attempts to use the conceptual schemata of ego psychology, psychoanalysis, Gestalt theory, and so on. The book is essentially a handbook on Rorschach writings and research, with an extensive bibliography for each chapter and a useful tabular comparison of the six major scoring systems. As such, the volume is probably of most interest to the prospective Rorschach researcher who is already quite sold on the technique and is conversant with its *lingua franca*.

Joseph H. Handlon

MUSSEN, PAUL HENRY, ED. *Handbook of Research Methods in Child Development*. Wiley, New York, 1960. x + 1061 pp., illus. $15.25

In 1946 this reviewer bought a copy of Wiley's newly published *Manual of Child*

Psychology (1068 pp.) for $6.00. In 1954 a second edition appeared; compared to its predecessor it was a little thicker (by 227 pages) and a lot dearer (by $8.00). Now, in 1960, Wiley has unleashed its *Handbook of Research Methods in Child Development*, and the price on the tag is $15.25. If one figures on a cost-per-page basis, Wiley is now charging 162 per cent more for its classics in child development than it did 14 years ago. (Wiley is also about three laps ahead of the average price increase for all commodities, since, according to the Consumer Price Index of the Bureau of Labor Statistics, the cost of living rose only 52 per cent during the same 14-year period.)

Faced with an outlay of $15.25, the potential buyer would do well to window-shop through the book first and to ask himself: Is the quality of this book as high as its price? Will it really be so useful that I shall need to have it within easy reach, or would it do just as well to depend on the library's copy?

If one browses through the preface first, one finds the editor's statement on the purpose and scope of the book. Its publication was proposed by the Child Development Committee of the National Academy of Sciences—National Research Council, because the members of the Committee felt that the quantity and quality of developmental research needed considerable improvement. The *Handbook* was intended to help rectify this situation, to be a representative, comprehensive and useful reference source of methodology in developmental research in the biologic and behavioral sciences.

In light of this purpose it is logical to ask if the finished product is really a handbook. That is to say, does it contain information that is immediately and genuinely useful in doing research, as does, for example, the *Handbook of Chemistry and Physics*? To be sure, there are some sections that contain ready-to-use information, such as Thompson's "Guide to the Observation of Group Behavior," or Baldwin's and Bijou and Baer's advice on the ways to hook and hold a young subject. And certainly the chapter by Wright on observational techniques is unexcelled in terms of potential utility. (It is also unrivaled on the score of scholarliness. As a matter of fact, between Lambert—whose chapter is really about observational methods too—and Wright, all has been said that might be said about the history, theory, content and techniques of observation. The two chapters combine into the kind of monumental discourse on observational methods that should withstand all competition for a good many years.) On the whole, however (and certainly when compared to the *Handbook of Chemistry and Physics*, edited by C. D. Hodgman and published in 1960 by the Chemical Rubber Publishing Company of Cleveland), this volume is more nearly akin to a book of readings than to a reference book. This is in no way meant to disparage the interest value of the contents, but simply to point out that the book editor does not, in one respect, live up to his avowed intentions.

Perhaps one of the reasons that this is not a true handbook or reference book is that it has tried to capture a wider audience than it can accommodate in 1061 pages. (And here one also touches on the point of comprehensiveness.) In view of the many separate fields in the biologic and behavioral sciences—anatomy, physiology, pharmacology, biochemistry, biophysics, genetics, zoology, sociology, cultural anthropology, psychology, and so on—an attempt to cover developmental research in each area is manifestly an unrealistic goal to set for a single volume. Certainly, developmental research in biology cannot be covered in two chapters, as has been tried here. (As a matter of fact, even psychology is not treated comprehensively, although it lays claim to about three-quarters of the territory covered by the book.)

To add misfortune to inadequacy, in this volume the relations between biology and behavior are about like those of a recently divorced couple who find themselves invited to the same cocktail party: they carry themselves as strangers, and make wide conversational detours around the experiences they have shared together in the past. And so it is in this book: the psychologists talk about psychologic psychology (aggressive fantasies), while Meredith, Macy and Kelly talk about biologic biology (cadavers as subjects of research), but neither group makes reference to the other. There is no discussion of developmental electroencephalography, of psychopharmacology, of the GSR (John Lacey's name does not even appear in the book). Nor is there discussion of other research techniques, of long-term popularity with both biologic and behavioral scientists, that have marked the commonalities, the points of communication and the understanding between the two groups.

Perhaps another reason the book falls short of its potential as a useful reference source for research methodology has to do with the difficulty scientist-authors apparently have in shaking off their habitual set to think and write in terms of substantive content or theory, and to focus instead upon methodology, drawing upon content and theory *only* as they contribute to methodology. The authors of the *Handbook* have met this challenge with varying degrees of success. A few, more excited by the way things came out than by what went into them, have turned out fare better suited to the *Manual of Child Psychology* than to a treatise on methods. Several others scutter back and forth between content and method like Lewinian children caught in an approach-approach conflict. Others (not very many) manage to keep the purpose of the book in clear focus, and avoid with commendable restraint the Lorelei of pure content and pure theory.

What has happened is that many specific matters of methodology have escaped entry entirely, and a few general aspects of methodology have, on the whole, been dealt with quite inadequately. In this second respect, the most outstanding oversight in a matter of general importance is the lack of concern with empirical questions of reliability and validity of measurement. In any other kind of book these questions are merely important—in a book about methodology they are essential, for a method without adequate reliability and validity is in the same sort of fix as a vertebrate without a spinal column.

By conservative estimate, more than one-half of the chapters in this book (there are 22) should have been conscientiously concerned with the reliabilities and validities of the methods they presented—that is to say, the literature on each method should have been dredged and all of the empirical evidence summarized. In addition, a word of explanation, advice or caution should have been posted when peculiar problems of measurement exist. Measured against these standards, in only three chapters are reliability and validity at least adequately covered, and presented in a way potentially useful for research purposes. These are the chapters by Wright (observational methods), Meredith (physical growth), and Bronfenbrenner and Ricciuti (personality). The more typical treatment of reliability and validity is a brief testimonial to their importance, or a paraphrased account of the American Psychological Association treaty of 1954, recognizing four provinces of validity and demarcating their boundaries. (This was published as a Supplement to Volume 51 of the Psychological Bulletin.) In some cases even this is lacking. The chapter on projective techniques, for example, has nothing at all to say about reliability and validity, an unfortunate circumstance in view of the fact that the projective literature is enormous and abounds with figures on reliability and validity, yet the projective technique with

reliability and validity beyond suspicion has yet to be invented.

There is another void of major dimension in the *Handbook*. Currently about 40 to 50 per cent of all research in child psychology makes use of subjects who are abnormal to some degree and in some specifiable way. (This estimate is based on information from the Bio-Sciences Information Exchange. The figure for countries other than the Untied States and for disciplines other than psychology would of course vary from that cited.) The representation of this research in the *Handbook* is fairly close to zero; the count is documented more precisely here in tabular form. (Similarly, topics related to or dependent upon abnormality are also lacking. There is no account of research methodology in any type of therapy or diagnosis.)

No reference:
- Abnormal (or psychopathology, etc.)
- Behavior disorder
- Psychoneurosis (or neurosis)
- Psychosis
- Psychosomatic disorder
- Sensory disorder or deficiency (except deaf-mutism and blindness, below)
- Glandular dysfunction
- Physiologic deficiency or dysfunction
- Anatomic defect

One reference:
- Blindness
- Deaf-mutism
- Brain injury or CNS involvement (except cerebral palsy, below)
- Mental retardation (or feeble-minded, exceptional child, slow learner, etc.)

Two references:
- Juvenile delinquency

More than two references:
- Speech defects
- Cerebral palsy

Unfortunately, research strategy is not independent of subject-parameters. Just as experimental variables, measures, procedures, etc., must be made appropriate to the type of organism, its age, and so on—so they must also be adapted to the organism's physiologic and psychologic state of fitness. It is not simply the content of a research problem that is likely to change when the subjects are hyperactive or crippled or mentally retarded children; it is fully as probable that a change must also be made in methodology.

Other omissions, briefly noted, are as follows:

1) A description and evaluation of research techniques with Important Adults—*i.e.*, all the parents, teachers, counselors and others, who so frequently appear in developmental research either as sources of influence on the child, or as sources of information about the child. As part of such a presentation, one would particularly like to see a definitive summing up of reliability, validity and utility of the guardian's report as a function of the type of information being sought and the particular methodologic circumstances under which the information is gathered.

2) An account of methodology in studies of learning under real conditions, or under laboratory conditions that have maximum generalizability to real situations. The most important of these are studies of learning in school or school-like situations. To have omitted this area seems doubly unfortunate, since the literature has been accumulating at a generous rate for over half a century—research on teaching machines alone is a literature in itself[1]—and because it contains lessons for methodologists than cannot be found elsewhere.

3) Contributions to research methodology

[1] See, for example, Fry, E. B., Bryan, G. L. and Rigney, J. W., eds. Teaching machines: an annotated bibliography. Audiovisual Communication Review, 1960, 8, No. 2, Supplement 1. Another bibliographic source can be found in Lumsdaine, A. A. and Glaser, R., eds. *Teaching Machines and Programmed Learning*. National Education Association, Washington, D. C., 1960.

from centers other than the United States and Geneva. For the book as a whole, the proportion of foreign-published bibliographic references to all references is only 0.06, and this would be even lower if it were not for the more cosmopolitan contribution by Inhelder and Matalon. With unabashed insularity, we tend to think that *research methods* and *American research methods* are identical; but in point of fact, research in child development did not begin in this country, nor have we, at any time in its history, been exclusively responsible for its methodologic innovations. The Soviet Union—to cite the most formidable competitor we have failed to notice— began publishing its research in child psychology and physiology fully as long ago as did the United States, has since the turn of the century been developing and using methodologies as yet untried or even unheard of in this country, and at present has surpassed the United States in the volume of developmental research proportional to the total amount of research in psychology and physiology (Brackbill, Y. Experimental research with children in the Soviet Union: Report of a visit. Amer. Psychol., **15**: 226–233, 1960). Surely it would be advantageous, in a revision of this book, to invite a Soviet physiologist and a psychologist to contribute a chapter, as Inhelder and Matalon have done on behalf of the Piaget school in this first edition.

All of the foregoing criticism has been made from the point of view of and in response to the stated intentions, purposes and goals of this volume. No mention has been made of the fact that, intentions aside, there are some very interesting and well written sections: Gibson on perception, Spiker and Bijou and Baer on conditioning, Thompson on dimensions of group functioning, Irwin on methods of studying sentence structure, Brown and Berko on an experimental approach to English morphology. Wright and Lambert on observational methods make an especially worthy contribution.

Yvonne Brackbill

LICHTENBERG, PHILIP, KOHRMAN, ROBERT and MACGREGOR, HELEN. *Motivation for Child Psychiatry Treatment.* Russell and Russell, New York, 1960. 220 pp. $5.00

Although the title is somewhat awkward, this is a significant volume for those involved in the psychiatric treatment of children. To date, little has been done in the systematic study of factors impelling a family to seek assistance for a child with emotional problems, or of factors impeding that step. Increasingly large sums are spent each year by communities on mental health and treatment facilities, but with little understanding of the forces which prompt people to seek this help. This book attempts to reconstruct and to study systematically the paths of 30 families to a child psychiatry clinic.

The research team consisted of a child psychiatrist and psychoanalyst, a clinical and social psychologist, and a psychiatric social worker. Thus, several frames of reference and corresponding techniques are represented. In their statement of purpose the authors indicate that their study of motivation for treatment may assist others in spotting the unmotivated family and perhaps suggest ways to increase motivation. They also state that this specific study will be used to test their general theory of motivation. It is on this point that the issue becomes cloudy.

Starting with what appears to be an approach to the problem in terms of field theory, they introduce psychoanalytic concepts which only confuse and distort the original purpose of the study, and would better have been omitted. The authors state that they emerge with a theory that bridges the gap between ego and social psychology,

but they are not very convincing. Indeed, in following their theory they become involved in some obscure and circular thinking.

The research procedure included five rating scales on information gathered from interviews, psychologic tests and questionnaires, and conferences with other agencies or therapists. The first device tabulated the interactions between the family and the persons consulted about the problem. From this they arrived at a measure of the forces which impelled or impeded a family's rate of movement toward the clinic. It is interesting to note here that they found contacts with private physicians and pediatricians tended to impede movement, whereas family contacts with schools tended to increase significantly the family's rate of locomotion.

A second scale measured the family's perception of therapy as being congruent with other goals held by the family. Other devices measured the parents' characteristic response to need development in the child, the family's perception of the social reaction of a family-clinic relationship, and the family's relationship to other social groups. Also rated was the mental health of each family. Reliability studies were conducted on each scale.

Although one might be critical of the theory presented, *Motivation for Child Psychiatry Treatment* is worth reading. The chapter on family beliefs about the nature of psychologic problems contains some illuminating material, and suggests sources of information that could easily improve predictions of motivation.

The finding of a positive correlation between family mental health rankings and motivation for treatment is not surprising. Workers in the field know that the healthier families expect to find greater satisfaction at the clinic, and tend to stay longer in treatment. Unresolved is the problem of the poorly motivated family, the one usually most in need of treatment.

Charles J. Leiman

KAIJ, LENNART. *Alcoholism in Twins. Studies on the Etiology and Sequels of Abuse of Alcohol.* Almqvist and Wiksell, Stockholm, 1960. 144 pp. Sw. Kr. 25:-, bound, 30:-

Clearly there has been a resurgence of interest in studying twins to learn about hereditary and environmental contributions to various behavioral disorders. Limitations of such studies have been pointed out by a number of people, but they have usually had a fixed model of twin studies in mind, one which simply compared the intrapair differences of monozygotic (MZ) and dizygotic (DZ) twins with respect to certain traits. Kallmann combined studies of twins with studies of their families regarding traits found in the index twins, calling this the "twin-family method." But most of the major twin studies of behavioral disorders relied entirely on clinical identification of the traits in question. With the growth of experimental and clinical psychology, a new armamentarium of tests and controlled procedures has become available and can be used to great advantage when the investigator has specific hypotheses in mind about the nature of the disorder, its etiology and its clinical course.

This monograph by Kaij is a fine example of how knowledge of the etiology and sequels of alcoholism can be advanced by skillfully raising specific questions about such matters and employing tests of twins to get the answers. Several methodologic flaws are obvious in Kaij's study, but he shows considerable sophistication both in understanding the nature of the flaws and in trying to get independent evaluations of the degree and direction of error introduced by them. The work is carefully planned and executed, as might be expected from a student of Professor Essen-Möller of Lund.

Some of the main findings are: MZ pairs are more similar in their drinking habits than are DZ pairs. Kaij is properly cautious in interpreting this finding. The finding of an increasing concordance rate with in-

creasing degree of abuse in MZ pairs as compared with DZ pairs suggested to him the existence of a *specific* genetic influence on the development of alcoholism. Among MZ twins, those with heavier alcohol consumption were *not* intellectually inferior to their more moderate co-twins, nor did any clinical psychiatric syndrome seem to be more common among them. However, the twins who drank more heavily did show less adequate responses in various Rorschach test categories than did their co-twins. Kaij concluded from test and history data that the mental abnormalities in some alcoholics should not be regarded as effects of alcohol, but rather that the organic syndromes had existed before the abuse of alcohol began.

This of course does not adequately summarize all the data. A number of tests of memory, learning, concentration, motor performance and motivation were administered to 32 MZ pairs to measure the effects of alcohol on these functions when heredity was controlled. Case histories of the MZ twin pairs are presented in synoptic fashion.

This study will be of value to those interested in alcoholism, as well as to those who want to learn how twin studies may be advanced methodologically and developed for future investigations.

David Rosenthal

MEZER, ROBERT R. *Dynamic Psychiatry in Simple Terms.* 2nd ed. Springer Publishing Company, New York, 1960. xi + 178 pp., paperbound. $2.75

This book, a product of the author's experience in teaching medical, nursing and social work students, attempts to portray the growth and development of the individual in the framework of psychoanalytic theory. It also includes brief discussions of the various mental and emotional disorders. The author's aim is to make psychiatry understandable by writing in very simple terms rather than in a more conventional textbook manner. The result is an elementary handbook suitable for use in teaching undergraduate nurses and psychiatric aids. However, the over-simplification makes it of dubious value in the instruction of medical or other graduate students.

Most teachers of psychiatry have difficulty finding a textbook suitable for their teaching program, and it is doubtful whether this primer will meet this need. However, anyone looking for a book for an elementary course in psychodynamic theory and psychopathology might find it of value.

There are certain inaccuracies, such as classifying the psychosomatic diseases as hysteria. The chapters on schizophrenia are particularly well done.

Virginia Huffer

WHITFIELD, I. C. *An Introduction to Electronics for Physiological Workers.* 2nd ed. Macmillan, London, and St. Martin's Press, New York, 1959. xi + 263 pp. $3.75

In these days of increasing specialization, instrumentation may form the best bridges between isolated disciplines. Since electronics play such a potent role in modern instrumentation, a fundamental knowledge of electronics is essential for the serious researcher and might very well be considered as necessary for any well educated twentieth-century man. Thus, a number of possible needs might be met with a book on electronics for physiologic workers. It could serve as an elementary introduction, a basic grounding in fundamentals, or a practical handbook for the construction of useful devices.

Unfortunately, this book serves none of these purposes. There are many popular works which furnish an introduction to electronics and are much more readable than Dr. Whitfield's book. For a basic grounding in electronic fundamentals, the

inexpensive *Radar Electronic Fundamentals*, available from the U. S. Government Printing Office, is still hard to beat. For up-to-date practical information, the standard electronic periodicals and the specialized *Psychophysiology Newsletter*, assembled by Dr. Ax at the Lafayette Clinic in Detroit, Michigan, are recommended.

Nothing lasts long as a practical handbook in this rapidly advancing field, and, indeed, Whitfield makes no attempt to supply practical circuit diagrams. Finally, British nomenclature in electronics is so different from American that this book could actually be confusing to the beginning student.

Enoch Callaway, III

Clinical Neurosurgery: Proceedings of the Congress of Neurological Surgeons, Miami Beach, Florida, 1959. Vol. 7. Williams & Wilkins, Baltimore, 1961. xvi + 278 pp., illus. $12.00

This seventh volume of *Clinical Neurosurgery*, devoted primarily to brain tumors, contains the proceedings of the ninth annual meeting of the Congress of Neurological Surgeons, and is dedicated to Dr. William J. German, who was honored as the Congress' guest speaker.

The first two papers by Dr. German provide a long-term follow-up of acoustic neurinomas and certain gliomas from the combined Cushing and Yale series. The long-term follow-up is of particular interest in evaluating the biologic behavior of these tumors. Incidence, signs and symptoms, treatment and post-operative course are tabulated for each group. In the third paper, Dr. German draws from his extensive personal experience to provide a view of the past, the present, and his thoughts concerning the future of neurologic surgery.

An interesting synopsis of electronmicroscopy of brain tumors is given by Dr. Leventhal. The differences between normal and malignant glial cells and the appearance of meningiomas and neurinomas as studied with the electronmicroscope are described and illustrated. Dr. Pomera and Dr. Goldblatt relate their experience with tissue culture of a human glioblastoma studied with time-lapse cinematography. Dr. Zimmerman reviews his studies of gliomas produced experimentally in mice. Tumors which closely resemble the various human gliomas have been produced experimentally and have been found useful for study of biologic properties.

The results obtained by Dr. Sweet's group in the diagnosis of intracranial lesions with positron-emitting isotopes are given. In a series of 1,085 studies, 80 per cent accuracy in tumor diagnosis was obtained using radioactive arsenic. The best results were obtained in meningiomas, glioblastomas and metastatic lesions; posterior fossa tumors were often missed. Dr. Taveras describes his technique of fractional pneumoencephalography, and illustrates the normal anatomy of the subarachnoid pathways and ventricular system and the changes seen in various pathologic conditions. Disorders of visual perception encountered in patients with cerebral neoplasms are discussed by Dr. Macrae and illustrated with case reports. Dr. Rasmussen and Dr. Blundell report a 17.1 per cent incidence of brain tumors in a series of 1,000 patients operated upon for focal epilepsy. Cerebral astrocytomas were the lesions most frequently encountered, and comprised 45 per cent of the tumor cases. The authors indicate that, in most of these cases, signs or symptoms of a focal nature were present, so that the tumor incidence is not representative of a general population of seizure patients. The management of the patient with onset of seizures in adult life is well outlined.

Dr. McCarty describes the operative techniques he has found most useful in the treatment of meningiomas. He recommends a free bone flap to prevent postoperative

bleeding, and has found an S-shaped incision useful for exposure of parasagittal and pontine angle lesions. Dr. Furlow reports his experience in the management of patients with tumors which have metastasized to the brain from the lung, breast, kidney and other structures. The clinical course, efficacy of treatment and indications for surgery are considered and compared in these groups. An interesting discussion of tumors involving the third ventricle is given by Dr. Kahn. Of 24 patients with craniopharyngiomas operated upon by the author since 1947, radical removal was carried out in 22 cases, with a primary operative mortality of 4.2 per cent. The importance of cortisone in the management of these cases and the postoperative complications which may arise are discussed in detail.

Dr. Taveras, in his discussion of the radiotherapy of brain tumors, expresses the opinion that all gliomas should receive radiotherapy. It is likely that many authorities in the field would not agree with this view, and the author does not seem to have substantiated his belief by the data presented for certain types of gliomas. Dr. Woodhall describes his technique and the results of chemotherapy of gliomas by isolated perfusion of the brain. These studies appear to indicate that the technique is feasible, although the chemotherapeutic agents currently available have not been effective in modifying tumor growth.

This volume, which of necessity focuses on certain aspects of the study of brain tumors, is well illustrated and is written by authorities. It can be recommended and should be of value to neurosurgeons and to those working in related fields.

R. Gordon Long

UTKIN, I. A., ED. *Theoretical and Practical Problems of Medicine and Biology in Experiments on Monkeys.* Pergamon Press, New York, 1960. vii + 276 pp., illus. $9.00

This compilation of papers reports some of the results of a quarter-century of intensive primate research carried out at the Institute for Experimental Pathology and Therapeutics in Sukhumi, U.S.S.R. The two major lines of experimentation deal with, first, investigations of the subhuman primate nervous system itself, and second, attempts to use spontaneous and experimentally caused diseases in monkeys as models of diseases in man. There are papers on motor conditioning and frontal lobe lesions, experimental neurosis, spontaneous neoplasms, measles, shigellosis, experimental hypertensive cardiovascular disease and a variety of other topics.

The papers have many commendable features. For example, the straightforward experimental approach to neurosis and psychosomatic diseases is refreshing, as is the attempt to conceptualize the neurotic process in neuropsychologic terms. Further, a highly developed insight into the various primate temperaments is apparent throughout the book.

However, the Western scientist concerned with the same problems will perhaps find the differences in methodology both prominent and disturbing. For example, there is a total disregard of statistical considerations in experimental design and data analysis. One looks in vain for control groups and tests of significance. As a result, the majority of papers present results which are interesting and suggestive, but difficult to evaluate. Again, none of the studies on brain function employed any technique of stimulation or recording, despite the fact that these techniques would be appropriate for a neuropsychologic approach to a variety of central nervous system problems.

Despite these shortcomings, the book has a breadth of outlook, and a view of behavior as essentially a biologic phenomenon, which for many readers will constitute its major contribution.

Bryan W. Robinson

LONGWORTH, CLIFTON T. *The Gods of Love: The Creative Process in Early Religion.* Preface by Edward Podolsky. Associated Booksellers, Westport, Connecticut, 1960. 273 pp., illus. $6.50

A book on early religions which assigns Homer to the period between 500 B.C. and 100 A.D. (p. 16), and abounds in other egregious errors, can scarcely be taken seriously. Evidence of scholarship is practically nil: few "factual" statements are documented, and of those few, several are based on questionable sources, some not even listed in the bibliography.

The bibliography itself includes a charming, if decadent, novel by Pierre Louÿs, but omits works of primary importance and relevance, and gives neither the date nor the place of publication of any book listed.

Since many statements are questionable and not supported by acceptable references, one cannot use this work as a source of data, especially as some material presented as *data* is actually unwarranted *reconstruction*—such as the purple prose describing the variety of temple prostitutes found in the ancient Near East.

The title itself is misleading. Many chapters deal with obscene carvings in medieval churches, where no "early religion" was practiced; also, these carvings usually have nothing to do with phallic or fertility cults, but simply satirize monks, and therefore permit the author to draw invidious comparisons between "immoral" Catholicism and "simon-pure" Protestantism.

There is also much ambiguous moralizing. In general, phallic symbols or cults in Greek religion are described as laudable representations of Love, while Sheila-na-gig in a Christian church is viewed as a symbol of Catholic corruption.

Such a book raises only two questions: Why was it written? Why was it published?

George Devereux

TRIDON, P. *Les Dysraphies de l'Axe Nerveux et de Ses Enveloppes Cranio-Rachidiennes. Etude Critique de Status Dysraphicus.* G. Doin et Cie., Paris, 1959. 235 pp., illus. 2.3000 fr.

This complicated and controversial subject—status dysraphicus—is developed under the following chapter headings: 1) "Embryology of the Nervous System and of Its Envelopes. Summary of Current Knowledge on the Dysraphias and the Inductive Processes Concerned in their Development." 2) "The Neural Dysraphias: Medullary and Cerebral." 3) "The Osseous Dysraphias: Vertebral and Cranial." 4) "A Critical Study of Status Dysraphicus."

Taking issue with some of the older views, Dr. Tridon feels that faulty inductive processes, *i.e.*, dysfunctioning "organizers," are responsible for the various neurocraniovertebral dysraphias and account not only for their development *per se*, but also for the reciprocal relations that exist between the neural and the bony defects. Reference is made to basic observations by Speman, Dalcq, Lehmann, Needham, Holtzer and others. Dysraphia is considered to be the common denominator in such states as anencephalia, cranioschisis and rachischisis, porencephalia, cranium bifidum and spina bifida, agenesis of the corpus callosum, cysts of the septum pellucidum, diastematomyelia, encephalomeningocele and myelomeningocele, dermoid cysts, atresia of the foramina of Luschka and Magendie, syringomyelia and hydromyelia, periependymal gliosis, Arnold-Chiari malformation, the Klippel-Feil syndrome, and congenital pilonidal cysts of the dorsal midline.

Each of these conditions is discussed concisely from the standpoint of pathogenesis in the light of the world literature and of the author's own conclusions. The relation of the dysraphias to such conditions as Lindau-von Hippel disease, Sturge-Weber disease, Friedreich's disease and visceral anomalies is also taken into account. Tables help to bring pertinent information

into perspective. Thus, in a four-page table, Dr. Tridon correlates, from the temporal standpoint, embryo-fetal age, length and stage of neural development with the neural dysraphias that occur when there has been failure of closure of the neural tube —*e.g.* at five weeks (12-mm. embryo), microcephalia and infundibulo-hypophysial agenesias; at six weeks (14- to 17-mm. embryo), microcephalia and optico-striobulbar agenesias; and at 10 weeks (40- to 50-mm. embryo), agenesias of the commissures, corpus callosum and vermis.

As an extensive and ably presented review of dysraphism, the monograph makes gratifying reading. Little of the author's own work is included. The book brings together much pertinent information in the field and proposes reasonable hypotheses as to pathogenesis. On this basis the French Académie Nationale de Médecine awarded Dr. Tridon the Prix Alfred Dutens for 1958.

Webb Haymaker

SHAPIRO, ROBERT AND JANZEN, ARNOLD H. *The Normal Skull, A Roentgen Study.* Hoeber-Harper, New York, 1960. ix + 257 pp., illus. $18.00

This book is the result of an elaborate roentgen study of the normal skull at various ages. It covers even the embryologic stages, the neonatal skull and the skull in children. The adult skull has been depicted in considerable detail. The authors have also attempted to deal with the wide range of variability in normal skulls. It is essentially an atlas of roentgenograms of the human skull. The text is limited to a detailed description of technique and discussions of the anatomy and the structures seen in the views depicted. It appears to be an excellent book by specialists who are enthusiastic about their work.

It should be valuable not only for undergraduate medical students, residents in radiology and neurosurgery, but also for physicians preparing for board examinations in radiology and other specialities. It may also serve as a reference book in radiology departments.

Frank H. J. Figge

ROBINSON, ALICE M. *The Psychiatric Aide: A Textbook of Patient Care.* 2nd ed. Lippincott, Philadelphia, 1959. xiii + 200 pp., illus. $3.50

The author, well known as an educator and administrator in psychiatric nursing, describes the newer concepts and skills in caring for the mentally ill in a textbook written in easily understandable language and communicating warmth and sincerity for the mentally ill as well as for those who care for them. The frame of reference of the book is the large and typically understaffed state institution.

This, the second edition, re-emphasizes the attitudes conducive to a therapeutic milieu for the mentally ill. It considers the aide as a person working under trying conditions, and focuses on the aide in his relationship to patients.

Although the book was written primarily to include the progress that has been made since the first edition of *The Psychiatric Aide*, the author deals with communication, industrial therapy, and remotivation technique very briefly.

Attitudes of the psychiatric aide toward the mentally ill, and the interpersonal skills of psychiatric nursing care contained in the first edition, were so valuable to the psychiatric aide as to make it a widely used and much-accepted reference. In the opinion of this reviewer the second edition fails to measure up to its predecessor.

Marguerite Termini

RIGNEY, FRANCIS J. AND SMITH, L. DOUGLAS. *The Real Bohemia.* Basic Books, New York, 1961. xx + 250 pp. $5.00

From time to time JNMD has invited and printed a double review of a significant and controversial book. We hope to do more of this in the

future, although there is no warranty that such reviews create a fair field and no favor by balancing one another. The two reviews which follow were not so planned; they were prepared independently through sheer accident and are published together—if for no other reason than to demonstrate a healthy diversity in the climate of editorial opinion.

The Editors

This description of the so-called "Beats" is timely, vivid and sympathetic. Furthermore, it provides the reader with a glimpse into the individual life histories of some of those who turn to this modern form of "Bohemia" as a way of life, and a glimpse at the history of the Bohemian movement in our culture as well. One finds that this has been part of the general struggle between conformity and rebellion in human life. There has been a widespread tendency to think of the Beats as something new in quality and intensity, whereas in fact their equivalent has played a role in our development for over a century. There is nothing new in the hostility to the Beat; but it is disconcerting to discover to what extent the hatred which is born of childish terror has shaped this struggle, where objective clarity and sympathy are so badly needed. Recent unchecked police brutality is disturbing to anyone who cherishes the American ideal, and should be an occasion for searching self-examination.

This report is more than a study of the social and individual psychologic factors which create a need for "the beat way of life" in some individuals. It provides insights into that world which lies somewhere between healthy creativity and psychologic illness. Usually this is inaccessible to study because the individuals who inhabit this world can often hold frank illness at bay for many years, and also because they often harbor a suspicious distrust of doctors and especially of psychiatrists. They fear that even to have their way of life explored might deprive them of their insecure hold on the creative potential they cherish. They rarely come for examination, and accept treatment reluctantly, if at all, only when they are forced to do so by the police or by an outspoken breakdown or by addiction. Consequently, students of the creative process seldom have an opportunity to study the Beat. This makes Dr. Rigney's achievement all the more remarkable. He was able to become almost a member of their community, to study them patiently, to observe them individually and as a group. He won their confidence, so that a fair sample of them shared their life stories with him. Some were even willing to undergo psychologic testing. To take these initial steps required courage, imagination, dedication and sound methodology, in an area for research the implications of which are of basic importance to all human culture.

Further studies of this twilight zone are needed if we are to fill a gap in our understanding of the creative process; but in the meantime our debt to Dr. Rigney is one we gladly acknowledge.

L. S. K.

It is, unhappily, almost axiomatic that when the techniques of a discipline are applied to an area of investigation not directly related to professional training, the yield, though it may be of passing interest, is rarely a significant contribution. So with this volume, in which a psychiatrist and a clinical psychologist role-play as sociologists and present an account of a study of the upper Grant Avenue Bohemian colony in San Francisco. Rigney, the psychiatrist, spent a number of months in this self-contained community, gaining social acceptance of a sort (he became known as "the psycho head shrinker") and interviewing members of the colony. Smith, the psychologist, administered a battery of tests to a sample of the "beats" and developed evaluations of some of the subjects.

The style of life followed by those groups of people more interested in creativity than

in social conformity is certainly worthy of study. Sociologists have for some time been observing the so-called beatniks, and of late years there has been much ado in the newspapers about this social phenomenon. It is not a new subculture, nor is there anything really new in the police brutality and other expressions of rejection that are quite dramatically reported here. In their determined repudiation of the value system of society-at-large, contemporary beats are in social continuity with earlier Bohemianism.

The material, largely a series of anecdotal case histories, is for the most part highly readable. But this investigation adds little to sociopsychologic knowledge; it embodies, in fact, so many methodologic and strategic deficiencies that it might almost serve as a negative example, a how-not-to-do-it primer for the application of clinical methods in field research. The time is past for studies that are purely exploratory, that do not seek answers to defined questions, that lack stated goals or hypotheses. And in this setting any number of rewarding questions could legitimately be raised: How do the value systems of these people differ from those of a conforming middle class? What are their characteristic defense mechanisms? Are the usual criteria for mental illness meaningful in this social setting? No such questions were put. Rather, the project was conceived in the broadest of terms, as an effort "to evaluate, both scientifically and personally, the nature of" this community (p. vii).

In a study of this kind, where the investigator is a participant observer, an elemental consideration is whether, or to what extent, the observer, by his own presence, works changes on the behavior he is examining. Beyond remarking the informality of their approach, the authors show little concern for this issue—although the impact of the investigator's presence is clearly attested in a variety of ways, *e.g.*, in the forthright citing of a line of a poem written by one of the subjects: "Dr. Rigney will tell the squares the truth" (p. xi).

It is equally basic that the investigator be at pains to be objectively detached in his observations. Here, instead, one finds the qualities of trust and friendship heavily underscored in the reporting. Rigney, whose deep involvement with the project is amply clear (he remained in the community for a year after the research as originally planned was completed, "broadening the study to include observations about creativity"), seems at times to be more interested in refuting the critics of the Bohemians (*e.g.*, p. 31), and in destroying the social stereotype of the beat as a constant drug user, than in reporting factual observations.

Understandably, the people recruited for psychologic examination (not a representative sample) proved to be poor test subjects. The paper-and-pencil questionnaires (MMPI, California Personality Inventory) generated notably more resentment than did the projective measures (Rorschach, abbreviated TAT). Although one might question whether the normative data are really meaningful for this group, the objective measures were scored in prescribed fashion, and arbitrary criteria were established for the MMPI profiles in order to divide the subjects into six categories. No evidence is offered that these categories ("tormented rebels," "passive prophets," "earnest artists") could be distinguished other than qualitatively. The projective materials, which would seem to offer a potential for greater yield in this kind of setting, were presumably evaluated according to accepted practice, but these results are not presented at all, and are summarized only superficially.

The book shows many signs of hasty preparation: it is poorly edited, and larded with inaccuracies. Examples: A poem by Joe Gould (p. 35) is cited as a production of one of the subjects studied. (Gould, certainly not a member of the group under investigation, did in fact write the poem; but